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Truing up a six-inch shell after the body and point have been fitted together.

MAKING MUNITIONS FOR THE ALLIES.—[See page 408.]

The Molecular Volumes of Liquids*

And Their Other Characteristics, Depending on Chemical Nature, Constitution, etc.

By Sir Edward Thorpe, C.B., F.R.S.

As soon as it was clearly recognized that homogeneous chemical compounds had fixed and definite compositions, capable of being expressed quantitatively and in terms of the atomic hypothesis, and that the weights of their unit volumes under standard conditions were equally fixed and definite, it was inevitable that, as the number of such compounds increased, attempts should be made to ascertain what relation, if any, existed between the two physical quantities. Moreover, as with the increase in the number and complexity of such compounds it became possible to classify them into groups, of which the members exhibited relations among themselves more or less well defined, depending upon chemical nature, it became of additional interest to trace the possible connection between chemical nature, as such, and the weight of the unit volume; and the inquiry became still further widened when precise conceptions concerning chemical constitution, structure, molecular arrangement, and the various problems of spatial chemistry became part of the current doctrine of the science. Stated broadly, then, the object was to discover the connection between the molecular weights of substances and the weights of their unit volumes under comparable conditions, or, in other words, their molecular volumes, and their other characteristics, depending upon chemical nature, constitution, symmetry, homology, etc.

For obvious reasons the inquiry soon became more particularly restricted to the case of liquid substances. In the first case, the material was sufficiently abundant to afford the promise of comprehensive generalizations; in fact, many of the questions raised by the inquiry could only be solved by the consideration of facts deduced from the study of liquids. And in the second place, it was easier in their case than in that of solids to establish what might presumably be properly regarded as valid conditions of comparison. The systematic study of these questions may be said to begin with the work of Hermann Kopp, whose results were published in a series of memoirs contributed to *Poggendorff's Annalen der Physik* and to *Liebig's Annalen der Chemie* at intervals extending over fifty years, viz: from 1839 to 1889. Strictly speaking, Kopp was not actually the first to broach the subject, but such tentative observations as were made before his time possess no practical importance and have only a very slight historical interest. This arose partly from paucity of material, but more especially from the absence of any well-defined basis of comparison, whereby the recognition of any possible generalizations became hopelessly obscured. It was of little use, for example, to determine the weight of the unit-volume of the liquids to be compared at some uniform temperature, say at the melting point of ice, or the mean atmospheric temperature, the conventional temperatures to which specific gravities are usually referred, since liquids are not necessarily under comparable conditions at these temperatures on account of their variable thermal expansibilities. Kopp imagined that a comparable thermal condition would be found at that temperature at which presumably the internal pressures are the same for all liquids, that is, at the temperature at which the liquid passes wholly into the state of gas or vapor, or, in other words, at its normal boiling point. Other bases of comparison may be suggested nowadays and possibly some of them may be preferable to that adopted by Kopp, but at the time he wrote no other suitable method was open to him and it is at least doubtful even now whether any other system would serve to reveal any more comprehensive or more definite relations. Be this as it may, practically all subsequent workers have followed Kopp's example in principle, while in some cases varying his particular experimental procedure. Kopp's method was to select liquids of assured purity and, of course, of known molecular weights; to ascertain their boiling points under standard conditions, the details of which he greatly improved; to determine their thermal expansibilities over a range sufficiently wide to enable the law of the thermal expansion of each to be ascertained so long as it remained a liquid under normal pressure; to find its specific gravity at some convenient temperature, and thence to calculate the weight of the unit-volume at the normal boiling point. This value divided into the molecular weight gave what may be termed the *specific* or *molecular volume* of the liquid at the normal boiling point, or at the temperatures at which the intermolecular pressures of all liquids are presumably the same, or at least very approximately the same. If the appropriate units are chosen, the molecular volume may be defined as the volume in cubic centimeters at the normal boiling points occupied by the molecular weights of the liquid expressed in grammes. Thus, to take a case: the normal paraffin heptane C_7H_{16} boils under standard conditions at 98.43°, has a relative volume at this temperature of 1.14111 (vol. at 0° = 1), and a specific gravity at 0°/4° of 0.70048. Then since the molecular weight of heptane is 100.16 (O = 16), its molecular volume, that is, the number of cubic centimeters occupied by 100.16 grammes of the hydrocarbon, at its normal boiling point is

$$\frac{100.16 \times 1.14111}{0.70048} = 163.16 \text{ cubic centimeters}$$

This volume may be legitimately assumed to be proportional to the real volume of the molecules together with the interspaces in which they (or the atoms) vibrate. That this assumption is valid may be shown by other considerations. Thus it has been established that the volumes of liquid compounds, however variable, at their normal boiling points are 1.5 times their volume at the absolute zero. Hence both volumes are proportional to the real molecular volumes.

Since the time of Kopp, and more especially during the last thirty or forty years, the subject has been experimentally studied by numerous investigators, among whom may be mentioned Zander, Buff, Schiff, Gartenmeister, Naubeck, Pinette, Dobriner, Elsässer and Lossen; and a considerable body of literature has been accumulated, mainly in the form of detached memoirs dealing with special aspects of the general problem. Until quite recently the most noteworthy attempt to co-ordinate this large amount of observational work, and to incorporate its main results into the systematic literature of the science, was made by Horstmann in successive editions of Graham-Otto's "Lehrbuch der Chemie"—a work not generally accessible to English students. In the autumn of last year, however, Mr. Gervaise Le Bas published a careful digest on the subject, as one of the excellent series of Monographs on Inorganic and Physical Chemistry which are being issued under the direction of Prof. Alexander Findlay.[1]

Mr. Le Bas is well versed in the extensive literature of the subject, and in his previous contributions he has already identified himself with its study. His present work has involved a very considerable amount of preparation, extending, as he informs us, over a period of eight years. His book shows him to be an acute, painstaking and well-informed critic, and it may be accepted therefore as the most authoritative exposition of the present condition of the subject that has hitherto appeared. We purpose in what follows to give a concise summary of the more significant additions to knowledge to which it leads.

Mr. Le Bas arranges the subject matter of his work, so far as it relates to the point of view under consideration, in the following groups: Hydrocarbons; Halogen compounds; Organic compounds containing oxygen; Sulphur, nitrogen, phosphorus, arsenic and antimony compounds; Miscellaneous compounds of other elements. Such a scheme of classification is probably the most convenient that could be devised with the material at present available, and it will be desirable therefore to adopt it here.

Of all known groups of liquids the one which may be presumed to lend itself best to the recognition of physico-chemical relationships is that of the *hydrocarbons*. To begin with, their number is relatively large, and they may be subdivided into well-defined classes, the members of which possess simple and progressive relations among themselves and to the members of other groups. Accordingly they enable the influence of homology, isology, structure, grouping, substitution and other constitutive changes to be traced more readily than in the case of any other comprehensive class of compounds. As a rule, too, the disturbing effect of "association" is less frequently observed among the hydrocarbons than in other groups of organic compounds, as, for example, in that of the alcohols and certain other oxygenated substances.

Much of what is to be understood by these terms was, of course, quite unknown to Kopp and his immediate successors, since it is for the most part the result of comparatively recent attempts to unravel molecular structure. Although they, no doubt, fully recognized that important clues concerning the connection between molecular volume and chemical composition might be expected to follow from the study of the liquid hydrocarbons, their want of knowledge as to the essential differences in molecular arrangement which may occur among the members of this large group led them to compare substances between which no true analogy exists, and from which no valid or rational deductions could be drawn. Progress under such conditions was therefore impossible. And this leads to the general remark that anything in the nature of sound, comprehensive generalizations concerning the connection of the physical quantity we connote by the term molecular volume and chemical nature, using that phrase in its widest sense, can only be expected to follow when we are dealing with groups of substances of which the structure and constitution are understood, or which are at least known to be related among themselves in a manner capable of precise definition. Not that this should discourage the accumulation of the necessary experimental material even in the absence of such knowledge. We agree with Mr. Le Bas that a knowledge of this particular physical constant should be regarded as necessary to the complete history of a chemical compound, even although under our present limitations we may be unable to interpret its full significance. As it is, experience has shown that this constant has served to afford a decisive clue to constitution when chemical considerations alone have given equivocal or contradictory results.

Although the actual numerical values of the molecular volumes given by Kopp for the particular hydrocarbons he studied have been found by subsequent observations to be substantially correct, proving the high degree of accuracy of his experimental work, the inferences he drew as to the fundamental values to be assigned to the respective atoms of carbon and of hydrogen are wholly invalid for the reasons already given, that he drew no distinction between hydrocarbons of essentially different constitution; hydrocarbons of open-chain structure, for example, being treated as strictly comparable with those of ring structure. Further investigation has established that Kopp's assumption that similar atoms have identical volumes in these two classes of compounds is erroneous. There is invariably a considerable contraction in the ring structure as compared with the open-chain hydrocarbon of identical molecular weight.

Kopp found that a difference of CH_2 in what he regarded as a comparable homologous series corresponded to a difference of 22 in molecular volume, and he deduced the separate atomic volumes for C and H in all compounds as respectively 11 and 5.5 or 2:1. It has now been shown that the normal values of C and H in all compounds of open-chain structure at their boiling points are respectively 14.7 and 3.7 or 4:1, and this relation is preserved at their critical points.

When the difference in constitution between aliphatic and aromatic compounds was recognized, the attempt was made to explain the contraction which occurred in passing from the open-chain compound to that of ring structure by assuming a different value for single linked and double linked carbon, while that of the hydrogen atom remained constant. Thus in benzine three carbon atoms were assumed to have the value 11.0, the other three having the value 14.0, while the volume of the hydrogen atom was uniformly 3.5.

There is, however, no necessity to assume that variable linkage has any influence on the ratio of the atomic volumes of carbon and hydrogen. The 4:1 ratio still holds good both at the boiling and critical points and at all equally reduced pressures. At the same time, in passing from the aliphatic to the aromatic class of compounds the atomic volumes of carbon and hydrogen do actually undergo contraction without the characteristic ratio being affected.

It is further established that the values thus obtained are equally true for unsaturated and saturated com-

*Science Progress.

[1] "The Molecular Volumes of Liquid Chemical Compounds from the Point of View of Kopp." By Gervaise Le Bas. (London: Longmans, Green & Co., 1915.) See also *Science Progress*, April, 1914.

pounds: the values for corresponding members of the *n* paraffin, olefin and acetylene series are simply dependent on their composition. This is contrary to the conclusion of Buff, who sought to show that the atomic volume of carbon was greater in unsaturated than in saturated compounds. The evidence that unsaturation exerts no special effect would, however, seem to admit of no further doubt.

The scope of this article will not permit of a detailed examination of the various constitutive effects which have been found to influence molecular volume, such as complexity, branching of the hydrocarbon chain, self affinity between side chains, cross linking, and double bonds, ring systems, etc. All the experimental data hitherto obtained bearing upon these questions have been summarized and discussed in Mr. Le Bas's monograph, to which the reader who desires the information must be referred. There is the less need for dwelling on certain of these points in an article of this character for the reason that the evidence is as yet very meager. Far more work is needed in several directions before conclusions of sufficiently well proved generality can be drawn.

What, however, has been stated will suffice to show the character of information which the study of the relations between chemical nature and molecular volume is calculated to afford. In the space which remains we will briefly touch upon the facts yielded by the study of the other groups of substances above referred to and incidentally state what appear to be the more interesting points in the present condition of knowledge on the question.

The values for the *halogens* in a state of combination, as calculated by Kopp, are not very dissimilar from the average volumes obtained from a discussion of much subsequent work, both on organic and inorganic compounds; and these values are not very different from their volumes in the free state. Few observations on fluorine compounds are, however, available on account of the difficulty of preparing these substances and their instability, especially in contact with atmospheric moisture whereby glass vessels are liable to be attacked. In the family of the halogens the atomic volumes increase with the atomic weights, but not proportionally. Their values appear to be affected, but only to a relatively slight effect, by constitutive influences, and they have practically the same value in aromatic as in alipathic compounds. There is, however, some evidence to show that in the chlorobenzines the position of the chlorine atom in the molecule is not without influence upon its volume. Far more work, however, is needed before the precise effect of this and other constitutive influences can be said to be satisfactorily made out.

The study of the molecular volumes of substances containing *oxygen* presents features of special interest. It was recognized by Kopp that the value of the oxygen atom was not uniform, but was influenced by the particular method of combination. According to him what we term hydroxyl or single linked oxygen had the value 7.8, whereas carboxyl or doubly linked oxygen was 12.2. These values are not very dissimilar to those now current. The interesting point is that Kopp was the first to detect the effect of what we now term constitution on molecular volume, and thus the first to familiarize us with the conception of its general influence. More extended inquiry has shown that in reality oxygen possesses a number of values depending upon its environment, position, function, etc., and limiting values are now known with approximate accuracy for the various types of combination, e. g., hydroxylic, ethereal, ketonic, etc., both in aromatic and aliphatic compounds.

Space will not allow us to pursue this particular matter at length, but we may point out as a matter of general interest its bearing on the question of the molecular constitution of liquid water. Water is known to be what is called an "associated" substance. A number of physical facts combine to show that the formula H_2O does not represent its nature as a whole under any conditions, so far as is known, so long as it remains liquid. Ordinarily water is made up of molecules which are polymers of H_2O, in amount depending upon temperature. Its formula therefore is $(H_2O)_x$ where x is greater than unity.

The determination of its molecular volume at its boiling point would seem to show that strictly speaking it is not a hydroxyl compound at all, but is, in reality, the first term of the Symmetrical Ether Series, just as hydrogen is "the vanishing point" of the paraffins.

Combined *sulphur*, like oxygen, has at least two molecular volumes depending upon circumstances similar to those indicated in the case of oxygen: one of these values is identical with the molecular volume at its boiling point in the uncombined state. The molecule of liquid sulphur is known to be very complex —probably S_{16} and S_8; that of gaseous sulphur is S_6 or S_2, depending upon temperature. A knowledge of its molecular volume throws considerable light upon the structure of the sulphur molecule and also serves to explain the phenomena of color and ease of decomposition.

Information as to the molecular volumes of the halogen derivatives of sulphur, and of its oxides, and of a number of organic compounds of sulphur also tends to elucidate their probable structural formula, and, incidentally, the varying valency of the sulphur atom.

A large amount of work has been done upon the molecular volumes of the different classes of *nitrogen* compounds, organic and inorganic, and a number of significant regularities have been detected which serve to throw light upon the structure of their members. It would exceed the space at our disposal to attempt to deal in detail with the mass of experimental material which has been accumulated. This has been sifted and discussed by Mr. Le Bas, and we must therefore refer the reader who desires fuller information to his treatise. Many illustrations might be given from the study of the nitrogen compounds of the value of observations on molecular volume in affording an insight into molecular arrangement and structural grouping.

The same remark applies with equal force to the liquid compounds of the other typical members of the trivalent series—*phosphorus*, *arsenic*, and *antimony*. Phosphorus, as in the case of certain other elements, would appear to have a much smaller molecular volume when free than when combined, and this fact is consistent with the statial arrangement of the phosphorus molecule, which is known from other physical considerations to be complex. The number of phosphorus compounds which are available for determinations of molecular volume is, of course, far less than in the case of nitrogen, as many groups of nitrogenous compounds have no analogues among the other members of the trivalent series. The greater number of the phosphorus compounds which have been examined are inorganic. They are of comparatively simple constitution, the results are consistent and offer little difficulty in interpretation.

As regards phosphorous oxide, Mr. Le Bas points out that its molecular volume might easily be found experimentally: apparently he is unaware that it was so found by the present writer and Dr. Tutton as far back as 1890 (*Journal Chemical Society*, 57 (1890), 559). The value thus obtained agrees with that calculated on the assumption that the ring grouping and consequent volume of the free phosphorus molecule (P_4) is preserved in P_4O_6, which seems otherwise probable, and that the oxygen atoms are singly linked and possess Kopp's value of 7.8. It is, of course, easy to construct structural formula for P_4O_6 (which is known to possess this molecular formula) complying with these assumptions, and consistent with its reactions.

Fairly accurate values for molecular volume have now been obtained for the greater number of the non-metallic elements, including the metalloids, and for the relatively few metals which furnish suitable liquid compounds. It is unnecessary to set out these values here or to show in greater detail how they vary with constitutive influences. But one or two considerations arise in connection with them. It was pointed out by the present writer many years ago that a periodic relation may be traced between them. This fact is referred to by Mr. Le Bas and has been confirmed and extended by him. In his work he gives a suggestive diagram which serves to illustrate it very clearly. From it he draws the following conclusions:

(1.) That there is a periodic relation between the atomic volumes of the elements.

(2.) There is a tendency for the atomic volumes to diminish in each series as the atoms increase in weight. The smallest occur at Group 7.

(3.) There is a general increase in the atomic volumes of the members of each group from Series 1 onwards, that is, in the direction of increasing atomic weight. This increase is usually 3.6 or some multiple thereof.

Mr. Le Bas concludes his work, of which the present article is a very imperfect digest, with a thoughtful summary of the present state of the theory of molecular volumes in which from limitations of space we are reluctantly unable to follow him. The *résumé* is, however, most suggestive and cannot but serve to quicken renewed interest in the subject. What is needed is fresh investigation directed in the light of modern conceptions of molecular physics and of constitutional chemistry. Any earnest worker in physical chemistry who may be in search of a fruitful field of inquiry will find in this subject abundant opportunity for the exercise of his powers, and he can have no more profitable preparation for his task than a careful study of the monograph with which Mr. Le Bas has so opportunely enriched the literature of science.

A Cause of Mustiness in Bread*

By A. M. Wright

During 1915 the author was engaged by a firm of bakers to examine some flour, which, it was alleged, had been the cause of mustiness in the bread baked from it. For the purposes of the investigation six samples of flour were taken; and of these, two, which liquefied nutrient gelatin in forty-eight hours at 20 deg. Cent., were examined bacteriologically by Dr. A. B. Pearson, pathologist to the Christchurch Hospital Board, who drew fresh samples under strictly aseptic conditions; as a result of his investigations, he reported that in the flours represented by the two samples noted above as doubtful, prolific growths of both *Rhizopus nigricans* and an *Aspergillus*, probably *glaucus*, were present. Subsequently pure cultures of the *Rhizopus nigricans* and of the *Aspergillus* were handed to the author for further investigation and the results recorded in this paper were obtained.

The samples of flour drawn by Dr. Pearson contained 10.6 and 11.1 per cent moisture and were thus quite normal in this respect; further, the samples were not caked, and in no way showed evidence of having been at any time in a moist condition. Portions of the flour containing the prolific growths of *Rhizopus nigricans* and *Aspergillus* were made into bread by independent bakers, and all such bread was found to be musty and unpalatable.

Laboratory experiments were carried out as follows: 1. Sound palatable bread was sterilized, and then mixed with an extract from the *Rhizopus nigricans* and the *Aspergillus* cultures, filtered through C. S. and S. hardened paper. 2. Sound palatable bread was sterilized and mixed with a fluid extract of the *Rhizopus nigricans* and of the *Aspergillus* cultures. 3. A check with sterilized bread was carried out.

All were incubated at 37 deg. Cent. for twenty-four hours, and it was then found in the results from (1) that the *Rhizopus nigricans* extract produced a marked mustiness, and slight sourness, in the odor and flavor of the bread; with the *Aspergillus* extract the bread was very sour in odor and flavor. From (2) similar results were obtained, with the addition that growths of *Rhizopus nigricans* and *Aspergillus* appeared in the dishes treated respectively with *Rhizopus* and *Aspergillus*. In (3) neither mustiness nor sourness developed in the bread; it was therefore concluded that *Rhizopus nigricans* is capable of producing mustiness in bread, while *Aspergillus* is merely productive of souring.

Subsequently experiments were carried out in order to determine what enzymes were present in the *Rhizopus* and the *Aspergillus*, which might be responsible for the results noted. An extract of each culture was prepared by mixing the cultures with sand and triturating the mass with water; after filtering the solution through absorbent cotton, the filtrate was used for the demonstration of enzymes. The methods used were mainly those described in some detail by Houghton,[1] the following enzymes being tested for in each filtrate: lipase, invertase, diastase, catalase, and proteolytic enzymes. In the case of the *Rhizopus* extract, lipase, catalase, and a proteolytic enzyme were found; in the *Aspergillus* extract, there were found lipase and invertase; in neither extract could diastase be demonstrated. A. Percy Smith[2] has shown that *Mucor mucedo* is a responsible agent in the production of musty bread, and has noted that putrefaction of the affected bread is also associated with mustiness. In the case under review no putrefaction of the bread was found in the sense at least that it became foul and putrid in odor even after fourteen days; the bread certainly was unpalatable. The *Rhizopus nigricans* extract when mixed with gluten produced after twenty-four hours at 37 deg. Cent. an odor similar to musty bread, but no putrid odor could be noted.

From a consideration of the data presented above it appears clear that *Rhizopus nigricans* is a cause of mustiness in bread, and that the proteolytic enzyme present in this organism probably brings about the changes in the bread which are associated with the odor and flavor noted as mustiness.

Twin Co-Axial Screw Propellers

Experiments made in Germany with two screw propellers, for ships, one behind the other, show that but little increased speed is obtained by the arrangement; and rotating the two screws in opposite directions, either at the same or different speeds, has little effect on the results. It is, however, claimed that the double screw gives better control of the vessel.

*Sydney Section, Society of Chemical Industry. Reported in the *Journal* of the Society.

[1] *Journal Ind. Eng. Chem.*, 3, 505.

[2] *Analyst*, 10, 181-183.

The Problem of the X-Rays*

How an Important Question of Physical Science Has Been Solved

By Prof. Frederick Soddy, F.R.S.

THE title of this book[1] connects two branches of science that have not been previously connected, but its subject matter is an essential link in the connection between, not two branches, but almost the whole range of modern physical science. There never was any doubt about the nature of the electric waves discovered by Hertz, now, in wireless telegraphy, so powerfully applied to the arts of war and peace. A simple magnification of the scale, a multiplication of wave length from ten thousandths of a millimeter to meters, and the mind passes easily from the old to the new, from light to electric waves. A magnification of the scale is easy to conceive and easy to put to the test, but the opposite process, though easy enough to imagine, is by no means so easy experimentally to put into evidence. Fraunhofer's original diffraction grating consisted of fine silver wires wound regularly upon a frame, and with this rough instrument he diffracted and measured for the first time the wave length of sodium light. A grating capable of diffracting electric waves, on the other hand, would have to be an enormous structure in which the width of the space between the wires was comparable with the wave length of the waves.

An extension of the scale of known wave lengths, similar to that in passing from light to the waves of wireless telegraphy but in the opposite direction, occurred at about the same time as wireless messages first began to be transmitted through the ether. But many years had to elapse before this new excursion of science into the region of the infinitesimal was clearly understood or before the new rays deserved any other than their original name, signifying, in algebra, an unknown quantity.

The modern grating of Rowland used for the diffraction of light waves with its five thousand or more lines to the centimeter, ruled by a diamond on a glass plate with perfect regularity, is rightly regarded as a triumph of mechanical art, beyond which it is impossible for art to go. Yet something ten thousand times finer had to be found before the X-rays were to belie their name and take their right place in the gamut of electromagnetic radiations, as far removed from visible light waves in the direction of smallness as these in turn are in the opposite direction from those of the waves of wireless telegraphy.

For seventeen years after they were discovered by Röntgen, the real nature of the X-rays was discussed and left undecided until Dr. Laue, of the University of Zürich, in 1912 conceived the idea of employing a crystal, with its marshaled ranks of atoms, packed in regular order, many million to the linear centimeter, to do for X-rays what the diffracting grating does for visible light. The success of the attempt opened a new era of activity, not for one but for many sciences, and the first chapter of this new era in the science of crystallography is set forth in these pages.

It is fitting that in this work the pioneers in this country should have been Prof. Bragg and his son. The γ-rays of radioactive bodies, and what are in effect artificially generated γ-rays—the X-rays of Röntgen—are old acquaintances of Prof. Bragg, who turned to them, after his distinguished elucidation of the problem of α-ray transmission, for fresh worlds to conquer. For many years he labored to establish a view of their nature which is now mainly of historical interest, but it was the discovery of Laue and his colleagues which finally directed the inquiry into its present channels.

The problems involved have a general resemblance to those of the diffraction of light by a ruled grating, but are considerably more complex, for, in the crystal, instead of lines ruled at equal spaces in a plane, we have to deal with points or atoms orientated in definite but unknown manner in three dimensions of space. At the outset both the wave lengths of the X-ray and the distances between the diffracting points of the crystals were quite unknown, but very soon both unknowns were determined in absolute measure. A great simplification of the problem was effected by the younger author at the outset. He showed that, since each atom of the crystal emits spherical diffraction pulses, the latter must resolve themselves into a *reflected* wave. This reflection is quite independent of any polished surface, but is an interior effect depending on the existence in the crystal of parallel and definitely spaced planes of atoms. A crystal may in imagination be bisected by any number of planes in every conceivable direction, but certain of these, namely, those, in general, parallel

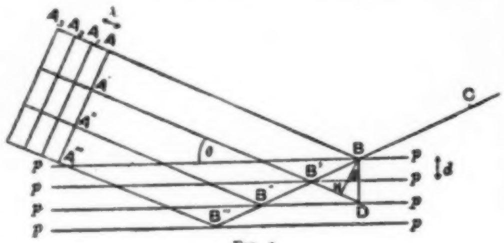


FIG. I.
Fig. 1.

with the natural faces of the crystal, will be crowded with atoms to a much greater extent than planes taken at random. Fig. 1 shows a beam of X-rays of wave length λ falling at an angle θ on a natural face of a

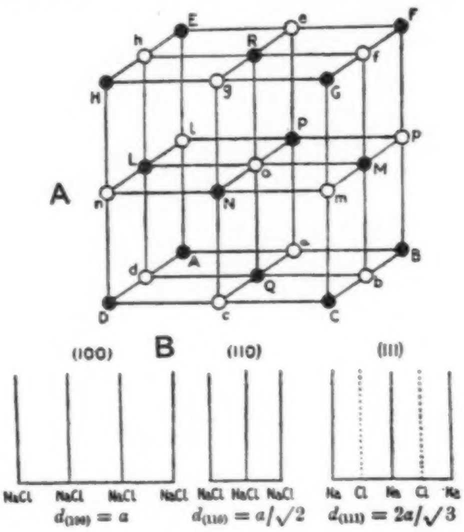


Fig. 2.

crystal, which consists of parallel planes, p, p', etc., spaced apart a definite distance d. The reflected waves from the lower planes add themselves on to and augment those from the upper plane only when the length of their path, from the line A, A', A'', A''', say, to C, is some whole number of wave lengths longer than that reflected from the surface. Otherwise they are in different phases, interfere and cut each other out. In short, the path of the ray from each plane must be the length of the line ND, which is $2d \sin\theta$ longer than that from the one above it, and therefore $2d \sin\theta$ must be some integral multiple of λ, the wave length. According as this multiple is 1, 2, 3, and so on, we have reflections of the first, second, and third order, much as in the diffraction grating.

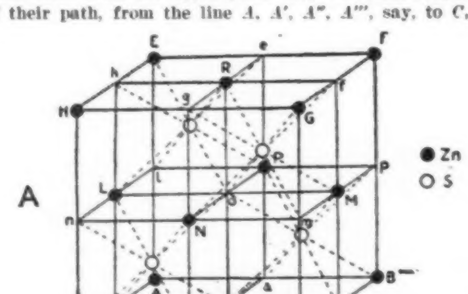


Fig. 3.

A crystal imagined to be slowly rotating from an initial position in which a beam of X-rays, of definite wave length, just grazes its face, will reflect X-rays intermittently as its face makes the angles θ_1, θ_2, θ_3, etc., with the beam. The sines of these angles, or the angles themselves approximately if, as is usually the case, they are small, are in the simple ratio 1, 2, 3, etc. With the same X-rays another face of the crystal with a different spacing d' between the planes will reflect at another set of angles θ'_1, θ'_2, θ'_3, etc. Nothing else is required to determine directly the ratio d/d' of the spacing in different planes. This is generally known from crystallographic considerations, and the direct verification of the latter is easy to carry out, and furnished one of the first proofs of the theory.

On the other hand, if different X-rays but the same face of the crystal be used, the ratio of the wave lengths of the two X-rays at once follows. But the absolute determination of both λ and d is possible when a complete knowledge of the structure of any one simple crystal is obtained.

For isomorphous crystals and for similar planes the spacing d should vary from crystal to crystal proportionately to the cube root of the molecular volume, and this was found to be the case for a whole range of cubic crystals. From this molecular volume and the mass of the hydrogen atom, these distances d can be computed, and consequently λ also. Thus, in rock salt, the distance between the successive planes parallel to the (100) face was found to be 2.8×10^{-8} centimeters, and the wave lengths of the two strong characteristic X-rays of rhodium, for example, 0.607 and 0.533×10^{-8} centimeters, i. e., 0.607 and 0.533 Angstrom units. Compare this with the wave lengths of the visible spectrum, from 7,000 to 3,500 Angstrom units, and this again with the waves used in wireless telegraphy, half a kilometer or more in length, and ponder for a moment on the incalculable service this extension of the scale of radiations has incidentally rendered to humanity. What unexplored stretches in this vast gamut yet remain blank! What secrets may still lie hidden within so vast a range!

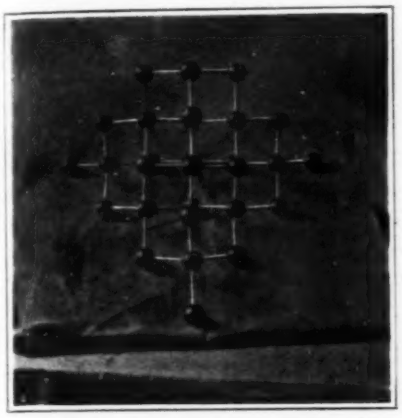
It has been too often the reproach of the otherwise well trained scientific man that crystallography remains to him a sealed book, a science with the meaning even of its nomenclature he is unfamiliar, a science of one too many dimensions to be easily pictured in what, owing to the fatal facility of scribbling upon paper, is in danger of becoming a two-dimensional mind. The authors have met this difficulty frankly and well, and it would be difficult to find anything of the kind more excellent than Chapter V, dealing with the rudiments of crystallographic principles. The labor of assimilation is lightened by numerous well executed drawings of solid models, some of which, by the courtesy of the publishers, are here reproduced. On the other hand, the complementary task of giving the crystallographer the necessary introduction to the science of the X-rays is less successful. For example, we read in Chapter IV—"The next table is well known." Yes! but to whom? One unfamiliar with the subject is plunged into very intricate phenomena at this point, with the minimum of help and guidance.

The first application of the new method made to the problem of crystal structure at once showed its power. A set of isomorphous cubic crystals, sodium chloride or rock salt, $NaCl$, potassium chloride or sylvine, KCl, potassium bromide, KBr, and iodide, KI, gave, by reflection from the (100) face, a set of values for the various crystals of d, the spacing between the planes, proportional to the cube root of the molecular volumes of the crystals. But potassium chloride alone gave for the spacings for the (100), (110), and (111) faces the ratio $1 : \sqrt{2} : \sqrt{3}$, required of a simple cubic space lattice. An examination of the other crystals showed that potassium chloride was apparently simpler than the other salts because its constituent atoms, potassium and chlorine, being nearly identical in mass, act indistinguishably towards the X-rays. For the other salts the behavior was completely explained on the view that

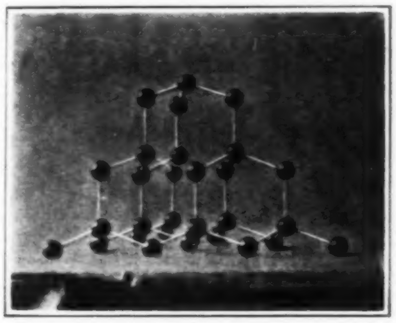
*From *Science Progress*.

[1] "X-Rays and Crystal Structure," by W. H. Bragg, M.A., D.Sc., F.R.S., and W. L. Bragg, B.A.

each of the two sets of atoms, of alkali metal and halogen respectively, lie on two different face centered cubic space lattices intersecting in such a way that when the two sets of atoms are alike the form reduces to the simple cubic space lattice. This showed at once that it is the individual atoms, and not the molecules,



Horizontal and vertical planes perpendicular to the paper are (110) planes.



Horizontal planes perpendicular to the paper are (111) planes.

Fig. 4.

which act as the diffracting points in the crystal.

This set of crystals is depicted in Fig. 2. The black and white dots represent the two kinds of atoms. The (100) planes are those parallel with *ADHE*, the (110) planes are parallel to that containing the points *CDEF*, and the (111) planes are parallel with that containing the points *BDE*. It will be seen that in the (100) and the (110) planes the atoms consist of equal numbers of both kinds, but the (111) planes consist alternately of all halogen and all alkali metal atoms, the one kind of plane being midway between two of the other kind. This is shown in the figure in the diagrams below the crystal model.

Another cubic crystal, zincblende, ZnS, was found also to have its zinc and sulphur atoms respectively in two separate intersecting face-centered cubic space lattices, but the mode of intersection is different from the former case (Fig. 3). The one set of atoms occupy the centers of four out of eight of the eight small cubes into which the large cube is divided. Or, what may not be so immediately obvious, each atom of either kind occupies the center of a regular tetrahedron, with four atoms of the other kind at the four corners of the tetrahedron. This is best seen for the tetrahedron *DLNQ* of the figure.

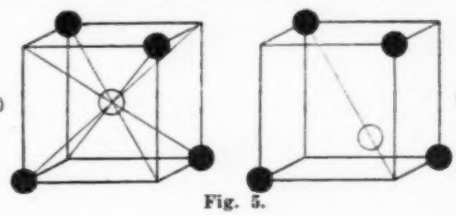
The next crystal, the diamond or crystal carbon, is one of the most instructive. Its structure is identical with that shown for zinc sulphide, with this difference, that both sets of atoms are now alike. Hence crystal carbon is built up out of atoms of carbon so orientated that each atom occupies the center of a regular tetrahedron the points of which are occupied by four other carbon atoms. One can hardly appreciate too highly the genius of Lebel and van't Hoff, who, without any of these direct and powerful weapons of modern physical investigation, arrived forty years ago at this conception of the spatial relations of the carbon atom from the constitution of optically active organic compounds. Nor, indeed, must the patient work of the mathematical and experimental crystallographers be forgotten. Working solely from the external forms of crystals and the general principles of solid geometry, they prepared the way for these striking and rapid advances.

Other representations of the structure of the diamond crystal, built up out of the ordinary model carbon atoms of the organic chemical laboratory, are shown in Fig. 4. The peculiarity of this structure from the standpoint of the X-rays is well seen in the lower figure. The (111) planes of the crystal are arranged in pairs with a gap between twice that between the components of a pair. This results in the entire suppression of the second order spectrum in the reflection from this plane, and of the first order spectrum in the reflection from the (100) plane, through interference.

Of other interesting cubic crystals, calcium fluoride or fluorspar, CaF_2, gives X-ray spectra analogous to those given by the diamond. Here the weight of the two fluorine atoms is very nearly the same as the weight of the single calcium atom. Its structure is that of zinc sulphide, with the difference that there is a fluorine atom in each of the eight small cubes of the figure. In iron sulphide or pyrites, FeS_2, another cubic crystal, the structure is similar to that of fluorspar, except that the sulphur atoms no longer occupy the center of the small cubes, but a position on the diagonal joining one of the iron atoms with the unoccupied corner of the small cube, approximately four times as far from the iron atom as from the unoccupied corner. In Fig. 5, (*a*) represents the arrangement for fluorspar and (*b*) for pyrites.

The authors then go on to the consideration of more complex crystals, of which the examination has already been begun, but these examples must suffice. To do full justice to such a subject would require a mastery of many branches of science not yet interlinked. "We have refrained from the discussion of a number of interesting points of contact with other sciences"—it is stated in the preface—"and with older work, such as for example the remarkable investigations of Pope and Barlow." Tutton's work on isomorphous crystals, and his clear distinction between eutropic series and iso-

(*a*)



(*b*)

Fig. 5.

morphous series, is recalled by the following passage: "It is interesting to notice that ammonium chloride is in no sense isomorphous with the other alkali haloids, although it crystallizes in the cubic system. Each unit cube of the structure contains in the one case half a molecule of KCl and in the other case a whole molecule of NH_4Cl." There are other cases also in which workers in allied fields must supply for themselves the due coordination of the old and the new, a monopoly of attention being, naturally, lavished upon the latter. But this, perhaps, is inevitable in a subject with so many points of contact with others. It is probable that the work both on its systematic and theoretical side is only in its beginnings, and the book will undoubtedly do much to stimulate the most profitable of all scientific work, the correlation of the various branches of knowledge.

Proportional Reducers

Photographic reducing solutions may be divided into three classes: (1) those which attack the highest densities most, (2) those which attack the lowest densities most, and (3) those which reduce all densities in the same proportion.

Sensitometric strips of various plates were exposed, developed and reduced in a thermostat under an accurately controlled condition, the plates being first developed, fixed, washed and dried, and then, after measurement, reduced, dried and read again. The percentage of the original density removed by reduction from each step of exposure was plotted against the logarithm of the exposure. The results show that persulphate is the only reducer which removes a greater percentage of the higher densities; and that all others tried, including some which have been supposed to work proportionally, really reduce the lower densities most, so that they tend to remove the shadow detail, the reducer having the greatest influence in this direction being a solution of ferricyanide and thiosulphate, which reduces the shorter exposures to such an extent that its effect on the curve through the greater part of its action is exactly as though less exposure had been given to the plate, this reducer being very useful for correcting overexposure.

After a trial of various proportions of the permanganate and persulphate mixture suggested by N. Deck, the following formula was adopted as giving the most evenly proportional reduction:

Solution A:

Potassium Permanganate....	0.25	gramme
10 per cent Sulphuric Acid..	15	cubic centimeters
Water	1,000	cubic centimeters

Solution B:

Ammonium Persulphate.....	25	grammes
Water	1,000	cubic centimeters

Use 1 part of *A* to 3 of *B*. The solutions will keep well and should not be mixed until ready for use. The time of reduction required will be from one to three minutes, depending on the amount desired. Reduction should be followed by immersion for five minutes in a 1 per cent solution of potassium metabisulphite. The plate should then be washed for a short time.

In the course of this work it was observed that the result obtained depended to a considerable extent upon the nature of the emulsion used, even this proportional reducer showing greater action on the low densities in the case of a slow, fine-grained plate.—*Kenneth Huse and Adolph H. Nietz, Research Laboratory of the Eastman Company.*

Stepped Ignition of Gases

At the recent meeting of the Chemical Section of the British Association, Prof. W. M. Thornton, of Newcastle, gave a demonstration of his experiments on "Stepped Ignition of Gases." He pointed out that when the frequency of collisions between the particles (molecules or atoms) of the combining gas (hydrogen, methane, other hydrocarbons, etc.) and oxygen was varied by changing the proportions or pressures of the gas in the explosion vessel, the inflammability (measured by the least current, potential or capacity required to produce ignition) was found in many cases to vary suddenly, in a series of steps, either up or down. The effect seemed to be caused by a selective absorption of energy. When the collision frequency between the combining gases passed through successive multiples (or sub-multiples) of that frequency which gave perfect combustion in the mixture at atmospheric pressure, there was a sudden change in the amount or form of the energy which had to be imparted to the gas by the spark to start inflammation. The origin of this selective action was obscure. It was not chemical, i. e., not dependent upon the formation of different stable combinations at each step, nor thermal, for the temperature of a gas depended on its translational energy, which did not undergo a sudden change.

There remained the vibrational energy, giving rise to radiation, and the rotational energy. If the effect were due to radiation from the atoms, it was difficult to see why it should in some cases cause a step upward and sometimes a step downward, and why it should in either case change suddenly. But the rotation of a freely spinning body was greatly affected by its collisions with other bodies, which would adhere to it for a time; any action calling forth excessive spin would of necessity retard the rise of translational movement and thus of temperature. Evidence for the momentary existence of excessive spin might be found in the so-called suppressed temperature of an explosion, in the fact that the amount of radiation did not exceed twenty-five per cent of the whole energy of combustion, and in the reversed spectrum of an explosive wave, which, seen from the front, with the wave advancing toward the spectroscope, showed every line reversed, but seen from the back showed the lines bright and normal. Vibration was therefore suppressed at the moment of combination; the pressure being halved, there was only the excessive spin left to take up the energy of combustion. The mechanical conditions affecting rotation might explain the steps. Relatively inert molecules behaved like hard elastic spheres, but molecules which combined did not so behave for the moment of contact. If the collisions (with combination and extra spin) occurred before the excess spin of a former collision was reduced by neutral collisions, the rise of translational energy could be delayed. The steps would appear to depend upon the relation between these times of reduction of spin and of collision, and might, in that case, be more difficult to observe in heavy gases (pentane, gasoline vapor) than in light gases.

In demonstrating a step in a mixture of hydrogen and air kept at atmospheric pressure Dr. Thornton altered the capacity of the condenser circuit in steps of 0.01 microfarad, and he showed that, while mixtures were inflamed by the same discharge when the hydrogen percentage was raised from 20 to 27 and 28 per cent, the capacity rose suddenly when the hydrogen percentage increased from 29 to 30 per cent.—*Engineering.*

Psychology and Light*

The Effects of Light on Mental Functions

By Hugo Muensterberg

I AM fully conscious how very little we psychologists can contribute to-day to your important discussions and how great the honor is if a psychologist is called at all to mingle in your council. Only reluctantly does the laboratory psychologist venture to express opinions where experienced men of affairs deliberate upon the needs of the community. Yet he is encouraged by the rapid progress which applied psychology has undoubtedly made in the last few years. You all know that psychology in our present sense of the word, a study of the mind with the means of an exact science, with laboratory experiments and in intimate contact with the physiology of the brain and the nervous system, is a thoroughly modern science. The first regular workshop for experimental psychological research was not founded until 1878 in Leipzig. To-day a hundred such laboratories exist in this country alone. But while the advance has been rapid and while ever new fields of mental life have been conquered by the scientific and experimental methods, the work remained strangely afar from the concerns of practical life. In the first decades the psychological laboratory work seemed to remain a most impractical research of scholarly theorists. Nobody gave any attention to the fact that the material which those scholars analyzed and investigated, namely the human feeling and will, memory and attention, perception and judgment, sensation and emotion, were the material from which our daily life with all its human interests is built up. You as engineers can hardly imagine physicists closeted in their laboratories with their knowledge of steam power and electrodynamics and nobody asking whether that new insight might not be made useful for toiling mankind. But this was exactly the psychological situation up to the threshold of the twentieth century. In every schoolroom and every courtroom, in every hospital and in every industrial plant the human factor and the mental energies were the most important parts of the whole interplay; and yet neither the teacher nor the lawyer, neither the physician nor the manager, showed any interest in the new psychological discoveries.

The educational work awoke first. The teachers recognized the crying need to have a fuller grasp of the pupils' memory, attention, intellect and will. The medical men, too, for a long while looked with indifference at psychological progress. To-day the new tendencies can be felt in every corner of the field. Psychotherapy became influential, neurology was intertwined with physiological psychology and to-day the physician who diagnoses mental abnormalities and diseases can no longer afford to ignore the subtle experimental methods for tracing the disturbances of character and temperament, of intellect and of the mind's elementary functions.

It is only natural that the lawyers should have followed more slowly. The methods of law must be conservative. Yet the observations of the psychologist became so impressive that even the jurist had to listen. All questions of evidence have appeared in a new light since the psychology of testimony became a favorite topic of the applied psychologist and the motives of crime and the effects of punishment were at last seen in their psychological setting. The latter comers were the men of practical affairs. Everywhere, and surely nowhere more than in this country, the leaders of commerce and transportation and industry showed a wonderful eagerness to consult the physical and chemical and mechanical engineer, but even ten years ago they would have hardly listened to the strange message that they might also learn from the psychological engineer. But when finally the new hour struck, the advance toward psychology in the economic field was perhaps quicker than in any other. To-day the whole country resounds with the call for the psychologist in the business world. Every week sees new organizations for industrial or commercial psychology. The employers call psychological experts into their factories and mills and the employees join psychological courses. It began with an outer problem of the business world, the question of the effective advertisement. The advertising industry involves billions, and everything depends upon the right effect on the attention, memory, suggestibility and desire of the customer. Only the psychologist specializes in the study of these mental functions, and as soon as he began to experiment on the means of propaganda and display, the contact between the new science and business was established. But a larger problem pushed itself into the foreground. Through two well known movements, that toward vocational guidance for boys and girls when they leave school, and that toward scientific management in the factories, the interest of the community became focused on the need of selecting men and women for work with reference to their individual fitness, and that means first of all with reference to their mental traits. Experimental tests were worked out to determine quickly the individual differences of the minds and to correlate them to the particular needs of an individual position. The aim was to recognize the special type of attention or quickness of reaction or acuity of observation or span of memory or habit of learning or exactitude of impulse and so on before the man was appointed as motorman or as locomotive eningeer or as typesetter or as electrician. It is probable that the experimental study of individual fitness will remain the central problem of industrial psychology. At present surely we stand only at the foot of the ladder. But there remain plenty of other too much neglected questions which the psychologist must answer when they arise in the workshops, mills and factories. How is the technique to be learned? Above all, how is the technique to be adjusted to the general dispositions of the mind? The organization of motor impulses, the rhythm, the uniformity and monotony of work, the fatigue, the acquisition of habits in work, the influence of stimuli—everything suggests exactly the kind of analysis which only the psychological laboratory can furnish.

In the midst of this new group of inquiries the engineer must raise the question how light affects the industrial output. As yet we know far too little how the working man profits from changes in the illumination where he is working. Some of the scientific management engineers have secured marked improvements in certain mills by altering the position of the lights or the station of the worker with reference to the window, the intensity of the light or its diffusion. Yet all this has been attained by mere trying. We still lack any exact investigations. In the Harvard Psychological Laboratory we have approached the field with regard to different color effects and different intensities of light. One of my young assistants, Mr. Pressy, has been engaged for a long while in measuring the psychological effects of red, green, yellow, blue and white light, all of equal intensity. The subject remains for five minutes entirely under the influence of one color and then goes through tests by which his rapidity of tapping movements, his exactitude of pressure movement, his memory, his power of discrimination and other functions of import for technical life are measured. The results show marked individual differences. We find subjects with pronounced intolerance for a particular color, for instance one whose mental processes take twenty per cent more time under the influence of red or another whose memory work shows an unusual improvement under green light. But practically of more importance are the constant differences indicated by the averages from many subjects. The tapping activity shows the most uniform rate with green, is decidedly quicker with red and slower with blue. Arithmetical work is strongly improved by red, in a less marked way by an increase of brightness. A characteristic result of his investigation, which is not yet closed, is the independence of such objective results from the subjective feelings. The colored lights which are felt as pleasant produce by no means more favorable conditions for working efficiency than those which are felt as unpleasant. The industrial world will have to give much keener attention to the conditions of light in the producing plants and will have to consult the psychologist at every step.

But the illuminating engineers who inquire about the relation of psychology and light think less of the light needed for the manufacturing of technical products than of the light which their particular product, the lamp, is shedding. Of course, they did not wait for the psychologist to ask whether an illumination is agreeable or not, whether the lighted room is fit for readers or needleworkers, whether the lamps on the street are bright enough to protect against accidents. But whether such questions were answered by common sense or by reference to some text-books, in any case such queries about the visual impression reached only the periphery of the psychological problem. They dealt essentially with the psycho-physiological functions of the eye and had hardly any bearing on the higher mental processes. Even when the contact with the laboratory was secured the interest seemed confined to acuity of vision and to physiological states of the retina like the visual adaptation, contrast effects, and so on. This was natural from the standpoint of the illuminating engineer who begins, of course, with a purely physical measurement of the light intensity and to whom the impressions of the persons with whom he experiments offer only a kind of subjective supplement to the objective photometry. We have even heard the voices of opponents who warn the engineers against the introduction of such unreliable and fluctuating elements as mental measurements of illuminations instead of leaving everything to the strictly physical standardization. The experimental psychologist of to-day cannot take such a warning seriously. He knows that all illumination is made for mental consumption. The physical standards alone are right when the current is to give heat, but when it is to give light not for photographing but for illuminating purposes the human mind alone can furnish the standard. But the psychologist must add at once that the mind is not really consulted as long as only the acuity of vision and the light sensations as such are examined. He must rather emphasize, and all modern psychological knowledge backs his demand, that every illumination problem ought to be grasped in its whole psychological setting. Nothing reaches our eye which does not touch our whole personality. The visual impression is the starting point for a whole hierarchy of mental reactions. Every practical situation in which we use light demands more from us than mere awareness of the visual stimulus. Each time our perceptions and apperceptions, our feeling and our attention, our imagination and our will are involved. This is indeed the fundamental suggestion of the psychologist: Approach every one of your problems with due regard to the whole complex setting of the mind. The visual impression itself must always be regarded as a mere fraction of the effects which you secure. But secondly, remove these mental inquiries from mere haphazard judgments by a systematic turning to the laboratory experiment. The engineer knows well from his physical work that experimenting never means to reproduce the practical situation in the dimensions of reality. A little model on the laboratory table can furnish all the needed knowledge. It is not different with the mental operations. The psychological experiment too does not depend upon its being carried out under the actual practical conditions. Any artificial laboratory setting in which as in a miniature model the mental processes enter into play may be of service. Only under such simplified conditions can we vary the setting from test to test and study the resulting changes in the mental reactions.

Let me give you an illustration. Two years ago when the National Electric Light Association asked for my advice with reference to the street lighting problem, I at once wrote in my reply: "The mere possibility of visual discrimination does not insure comfort and still less safety on the street. The most essential point is to have an illumination by which the attention is kept vivid and all the mental functions active. Fair chances to see are of small use if the pedestrian or the driver come into a benumbed state in which his attention is dulled and in which his reactions are slow. Offhand and without having carried on any experiments whatever I should be inclined to say that a uniform illumination would be unfavorable for the attention. Our attention is naturally fluctuating and will best be kept awake if the illumination produces an alternation between tension and relaxation. This demands that there be darker regions between the lighted fields." Now a psychologist ought never to say anything "offhand and without having carried on experiments." I was, therefore, very glad when my assistant, Dr. Burtt, was invited to participate in those interesting experiments which were carried on here in New York in the summer of 1914 by the Joint Street Lighting Committee. We agreed that experiments on quickness of reaction time, both simple reactions and complex choice

*An address delivered at the mid-winter convention of the Illuminating Engineering Society, New York, February 10th, 11th, 1916, and published in the *Transactions* of the Illuminating Engineering Society.

reactions, ought to be made on the streets under different systems of illumination. A second set of experiments referred directly to the power of attention. The subject had to disentangle several geometrical figures out of a complex group of forms. And finally a test of motor co-ordination was arranged. The mechanical devices for all three tests were so constructed that the response of the individual was independent from the darkness or lightness of the spot at which they were carried on. The reaction was made to various sound stimuli and the forms to be analyzed were seen in a box with constant illumination. The results were objectively measured in hundredths of a second. The tests of motor co-ordination were registered by the signal magnet on the kymograph. The three tests were made on men who had walked through streets of either uniform or non-uniform illumination. All three tests together allowed a fair decision concerning a man's individual freshness and mental alertness under the two systems of light. You may have glanced over Dr. Burtt's report, and you may have seen how the results of his experiments, filling the fair evenings of six summer weeks, confirmed my expectations in full detail. His tables show that for instance the auditory choice reaction is, under the non-uniform illumination, in every case shorter and on the average 17 per cent shorter than under uniform lighting. The attention as indicated by the test with form analysis is under non-uniform illumination superior in twenty-six out of thirty-one series. Even the motor co-ordination was in 77 per cent of the series better developed under non-uniform lighting. But Dr. Burtt's report to the committee did not contain the second part of his investigation, which best illustrates my point. After completing these experiments on the street, he repeated the experiments throughout the last year under the artificial conditions of the Harvard Psychological Laboratory. There we equipped a black room with apparatus which allowed us to imitate the essential states of mind and at the same time to settle those questions which remained doubtful in the actual street tests. In practical life we cannot resolve the complex situation into its elements. In the psychological laboratory we can aim toward such a goal. Dr. Burtt asked himself, for instance, whether the superiority of the attention in streets with non-uniform light depended upon the fact that the pedestrian himself was alternatingly in light and in dark regions or whether it resulted from the outlook into a street in which light and dark strips succeeded one another. Again he had to meet the objection that non-uniform light on the streets produced better results in the tests because the man who passes a dark region voluntarily forces his attention to a higher pitch in order to discriminate the obstacles or irregularities on the surface. But the chief problem remained the difference between the two lighting systems. In the interest of the first problem our laboratory subjects, who are always post-graduate university students, reacted on sounds, analyzed complex figures and so on, while the light slowly grew dimmer and brighter alternatingly in the rhythm in which a man would pass from light to shadow in walking through a street with non-uniform illumination. To reproduce the second factor. the lights and shadows on the surface of the street, the illumination of the room was kept constant, but patterns of lights and shadows moved over the wall. In both groups of experiments the intensities, the pace of transition and the amount of the differences were kept quite similar to the street conditions. The results for all the laboratory experiments together substantiate the outcome of the street tests. The non-uniform light is more favorable for every type of tested mental activity. Auditory reaction time is superior under such lighting in 80 per cent of the series, visual reaction time is quicker in 65 per cent. The comparison between the effects of the moving strips of shadow and the rhythmical increase and decrease of light shows that much depends upon the speed. The score of attention is high when the shadows move in a cycle of eighty-five seconds, but low when they move as quickly as twenty-five seconds, while the alternation of strong and weak light in the rhythm of 25 seconds produces a decided heightening of the mental powers. I cannot enter into details here. I still want to mention only that Dr. Burtt's experiments made it clear that a voluntary re-enforcement of attention in the darker regions is not responsible for the superior achievement. For our purpose not the results are important but the method. If our laboratory tests can analyze the real elements which enter into the situation, it must be more advantageous to study the question under the pure conditions of the psychological workshop where every factor can be standardized and varied at will than on the street where the manifold conditions confuse the issues. As soon as the principles are recognized, it is not difficult to take account of all those disturbing elements by which the street differs from the quietude of the workroom.

But light not only serves for illuminating the surroundings. It may work directly on the eye as a signal. The whole railroad service depends upon this function. Again I may illustrate the help which experimental psychology can offer by pointing to a yet unpublished investigation in the Harvard Psychological Laboratory. Mr. Fry, connected with the Pennsylvania Railroad, spent the last year in our laboratory engaged in testing the different mental influences of railroad light signals on the mind of the engineer in the locomotive. We had secured material from all important railways of the country. Their confidential reports made it evident that among the cases of accidents traced to signal failures 85 per cent were caused by misinterpretations and only 15 per cent were due to the signal itself not working properly. This misinterpretation is a psychological process, which we had to study under all possible conditions. The levers of the locomotive were replaced by similar levers in a dark cabinet, each connected with electric markers which made it possible to read the time between the flashing up of the signal and the reactions of the hands in fractions of a second. We had our semaphores, we had our white and green and yellow and red lights in different positions and in different combinations. Long series of signals could follow one another as they would appear to an engineer during a long night trip. You may say that the complexity of the actual situation on the road is much greater and the demands upon the mind therefore much subtler than in a protected laboratory room. There are rains and fogs; but we were able to imitate their mental effects. Large smoked glass plates at different distances from the light gave us all that dimming which the fogs of various intensities could produce and we could study which signals suffered most from them. We tested the important improvements which have been proposed, the beams of light and so on. Above all, we studied again not simply the isolated light function but the whole mental reaction of the man with all his shades of memory and attention and training and fatigue.

The time allotted to me does not allow my entering into other illustrations, but there is no field of illumination work which has not its psychological aspect, and is not accessible to the methods of psychological experiment. The interior illumination with its problems of direct and indirect light is an especially fertile region for psychological study. How was it possible to introduce for indirect illumination in the home those lamps which are not translucent and create a big black disk in the midst of the lighted ceiling? They are disappearing to-day, but they would never have made their debut if the warning of the psychologist had been heeded. And with great skepticism he hears certain inside illumination praised because there are no shadows. He knows how much shadows help toward an easy grasp and an inner organization of the surroundings and through them to the comfort of the mind. And before I close, let me mention at least that world which the honored hero of this your meeting, Edison, has opened to us, the world of the film. We have been led from triumph to triumph there by the kinematoscopic technician. But only in most recent days has it been recognized how large must be the mental factor in the success of the moving pictures and how much of it is open to psychological experiment. We know now from such mental laboratory studies that some of the physical problems of producing the light for the screen were incorrectly put. The longest possible exposure of the picture, the shortest pause between two succeeding pictures, is not the most favorable condition for the strongest effect, although this appeared to the physicist a matter of course. Your physical instruments are wonderfully complex. But if it comes to a test the mind of man without doubt proves to be the still more complex instrument, after all. You have no longer any right to settle the psychological parts of your problems by common sense only. The bridge from the physical to the psychological laboratory is now firmly built, and whenever you cross it you will find a ready welcome in the camp of the hopeful psychologists.

Bitter Pit*

THE disease of apples (and pears) known as bitter pit manifests itself externally by depression of the surface of the fruit and internally by patches of discolored and dead tissue. It is a disease which may make its appearance while the fruit still hangs on the tree, or it may declare itself in the fruit-room and even in cold storage.

This disease has been, and still is, the cause of great loss to growers. Thus it has happened not infrequently that whole consignments of apples shipped from Australia to England have developed the disease so severely as to become unsaleable. Hence it is not surprising that so progressive a community as the Commonwealth of Australia should have instituted, with the co-operation of the state governments, a special research into the nature of the disease, its remedy and prevention. This research, endowed for a period of four years, was entrusted to Prof. D. McAlpine, and the fourth and final report now issued testifies to the assiduity and thoroughness with which both Prof. McAlpine and his colleagues have prosecuted their inquiries. As is pointed out in the introduction to the report, when the investigations which it summarizes were begun bitter pit was regarded as a mysterious disease. It is associated with the presence of no parasite, nor is it a consequence of puncture by insects of the skin of the fruit. Ewert had, it is true, advanced evidence in support of the view that bitter pit is a result of the local toxic action of copper-containing spray fluids. That hypothesis has not, however, met with general acceptance.

Our knowledge of the ætiology of this disease being so vague, we turn with interest and curiosity to learn the results of Prof. McAlpine's inquiries; but it must be confessed that although we discover much valuable and interesting information in this large and admirably illustrated volume, we fail to find the revelation of the mystery. The symptoms of the disease are described in detail; evidence is brought forward that severely pruned trees yield more pitted fruit than is produced by lightly pruned trees; that nitrogenous manures appear, albeit often to no considerable degree, to increase the pitting of fruit; that certain varieties are more resistant and certain others more susceptible to the disease—in fine, we learn much that is useful and suggestive, but of the cause or causes of bitter pit we are no wiser after than before the perusal of this monograph. We insist on this point with some emphasis because we think that it should have been made clear at the outset of the report, instead of which we find it there claimed that the research has been brought to a successful issue.

That Prof. McAlpine has made a definite and valuable contribution to our knowledge of this pathological problem will appear presently, but this is only an added reason why he would have done well to make it perfectly clear that the main problem still remains to be resolved. To conclude that the "immediate cause" of the disease is "the concentration of the cell sap" (p. 75) is not to discover a cause, but to use words the meaning of which is at least as obscure as the nature of the bitter pit. Moreover, if quick-acting nitrogenous manures, which lead to sappy growth, encourage bitter pit, how may that disease be attributed to concentration of sap?

Perhaps the most valuable part of Prof. McAlpine's studies is that which demonstrates the possibility of preventing the outbreak of bitter pit in cold-stored apples. As a result of experiment, he shows that if apples be stored at a temperature of about 30 deg. or 32 deg. Fahr., and if fluctuations beyond these limits be prevented, no bitter pit manifests itself during a period sufficiently prolonged to transport the fruit from Australia to Europe. This is a great gain, and the practical results accruing from it should not only pay for the cost of this elaborate investigation, but encourage the Commonwealth to promote further investigations into the origin of the disease.

A point of some interest on the scientific side of the problem is the fact that starch persists in the broken-down tissue of the pitted region of the apple pulp, whence it is concluded that the incipient but invisible stage of the disease occurs in the pre-ripening phase, or at all events during the phase in which starch gives place to sugar. This is plausible, but the opposite view is not precluded that the starch of the bitter pit arises as a result of a reconversion of sugar. In favor, however, of the view that bitter pit develops, although it is not apparent, at an early stage is the evidence obtained by subjecting suspected apples to X-rays, as a result of which it is claimed, and the claim is supported by photographs, that prospective pit areas appear on the radiographs.

Prof. McAlpine is hopeful that the loss due to bitter pit may be ultimately prevented by breeding pit-resistant varieties. It is a work worth undertaking, but nevertheless is not to be undertaken lightly, for it may prove a long business.—*Nature.*

*"Bitter Pit Investigation. The Experimental Results in Relation to Bitter Pit, and a General Summary of the Investigation." By D. McAlpine, appointed by the Commonwealth and State Governments of Australia. Fourth Report, 1914-15. Pp. 178+70 figures and colored prontispiece. (Melbourne: The Government Printer.)

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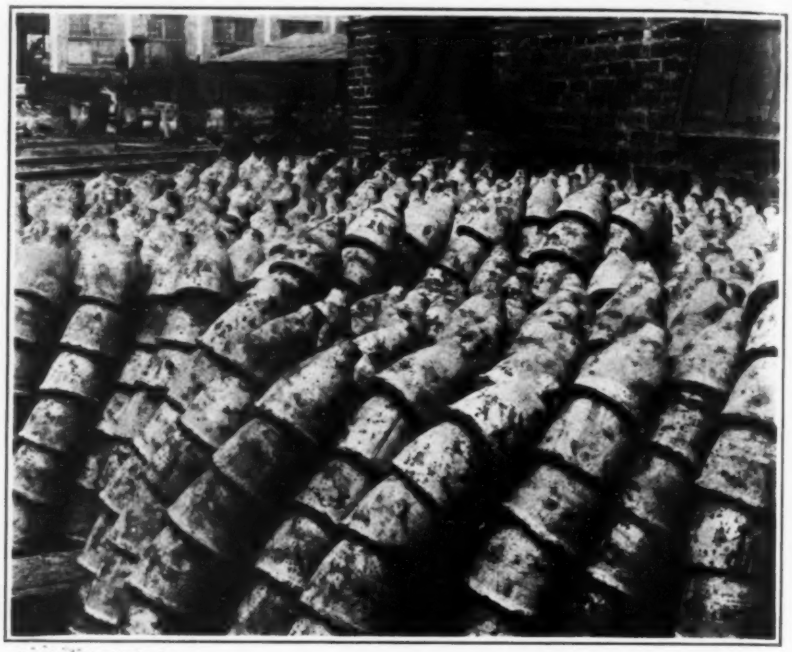
Rough forgings for making six-inch shells.



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Steel point castings for six-inch shells.

Making Munitions for the Allies

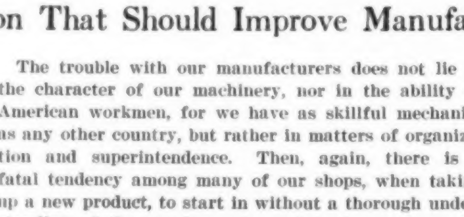
A Costly Lesson That Should Improve Manufacturing Methods

In the early days of the great war the American public was dazzled and astounded by the public reports of the contracts for enormous quantities of munitions, at unheard of prices, that were being placed with our manufacturers by the European allies, and it was regarded as quite natural and fitting that European countries, in their condition of unpreparedness and dire necessity, should turn to America, with its reputation for mechanical ingenuity and ability, and its great factories, for assistance. American manufacturing organization and ability was to be pitted against that of Germany, and the result was contemplated with complacency. American energy and efficiency was to show its superiority over the supposedly stereotyped routine of continental shops; but the actual results have been a humiliating surprise, in many instances, both to the public and to many an optimistic contractor.

These remarks apply particularly to the production of guns, rifles, high explosive shells, shrapnel, and the like, for when munitions are mentioned this is the class of material the public has in mind. Munitions, however, include a vast variety of other articles and materials, and in supplying these many companies have been eminently successful, for it was material with which they were familiar, and which they were regularly turning out before the war; but many companies, attracted by the prospect of big profits, and relying on their shop equipment, undertook the manufacture of unfamiliar products, in the way of arms and ammunition, only to meet with failure. Of course, there are many companies whose regular work was the production of arms, and these, for the most part, have been successful in making the needed supplies of the desired quality, but even in these establishments there have been some that have been carried off their feet by the unprecedented demand, and their inability to handle the immensely increased factories that they have hurriedly erected for the purposes of these special contracts. But the actual gross results of all these loudly advertised ammunition contracts has been practically insignificant.

One of the worst features of our failure to make good in the production of arms and ammunition of a satisfactory quality is the loss of prestige that American manufacturers will suffer after the war is over, when trade will be needed to keep all of our new mushroom plants going. International competition will then be keen, and the shortcomings of some inexperienced companies will become a telling argument against American producers generally, although we have the latent ability to turn out as good work as any country in the world; and as a matter of fact America does this very thing, as is evidenced by the fact that many of the noted establishments of Europe are largely fitted out with American machinery, which was bought on its merits, much of it before the war made additional equipment necessary.

The trouble with our manufacturers does not lie in the character of our machinery, nor in the ability of American workmen, for we have as skillful mechanics as any other country, but rather in matters of organization and superintendence. Then, again, there is a fatal tendency among many of our shops, when taking up a new product, to start in without a thorough understanding of the requirements, and to jump at conclusions. Another habit is to attempt to tell the buyer what he ought to have, instead of giving him what he wants, and in an exacting contract these methods of procedure often fritter away all the profits before the manufacturer realizes that he must give the buyer exactly what he indicates in his specifications. It may be allowable to advise and attempt improvements in articles that the manufacturer has been producing for years, and with which he is intimately familiar, but to undertake a new product successfully, it is absolutely necessary that the management should be thoroughly familiar with the broad principles of mechanical production, and then make a critical analysis of the particular product required. This is too seldom the course of practice, but rather the people responsible rely on the application of irrelevant methods that have grown up from experience in other lines, without any consideration of the underlying principles involved. While the results of our efforts to produce ammunition for our European customers, particularly shells and rifles, have been far from satisfactory, the experience should prove an object lesson of immense value to every manufacturer in the United States, if the underlying causes of these failures are correctly analyzed and the proper measures taken to avoid future repetitions of the mistakes.



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Inspecting shells and testing fit of nose-piece to body.

In the early days of the stories of big war contracts the companies involved were very reticent in giving out information of their manufacturing operations, partly because of the possible effects on the speculative money market, and partly because they didn't know themselves. Later on this secretiveness was found necessary to guard against the underground operations of secret agents of the enemy. Still much information has been forthcoming in regard to the doings of the large shops; and the accompanying photographs, which largely explain themselves, show a number of the actual processes in the manufacture of shells. The particular projectiles, whose making is here illustrated, are six-inch high explosive shells, used in the smaller naval guns and in field pieces. Of this class many different sizes are used, from 1.4 to 16 inches in diameter, in which the well-known T.N.T., or trinitrotoluol, is largely employed as an explosive. This, however, is but one of a number of explosives that have been used; and the Russians and Austrians are said to use a compound composed of from 12 to 15 per cent of trinitrotoluol, mixed with ammonium nitrate, as an oxidizer, a small amount of aluminium powder and a little charcoal. This is said to give better results than the plain trinitrotoluol, but has the disadvantage of readily collecting moisture. The British are using to some extent a similar mixture in which the above difficulty has been overcome, but not very much is known about the exact explosives now in use.

Mechanical Service in a Restaurant

In a restaurant maintained by a large manufacturing company for its employees an ingenious plan has been introduced to expedite the service and make it more convenient. This restaurant is run on the "self service" plan, and in front of the counter on which the various articles of food are displayed a belt conveyor has been installed. The patron places his tray on the conveyor, and walks along beside it, picking up the portions he desires and placing them on the tray as it moves along. When the tray is filled it is removed from the conveyor and taken to a nearby table. This device keeps the line of patrons moving steadily, and avoids the accidental spilling of a loaded tray.

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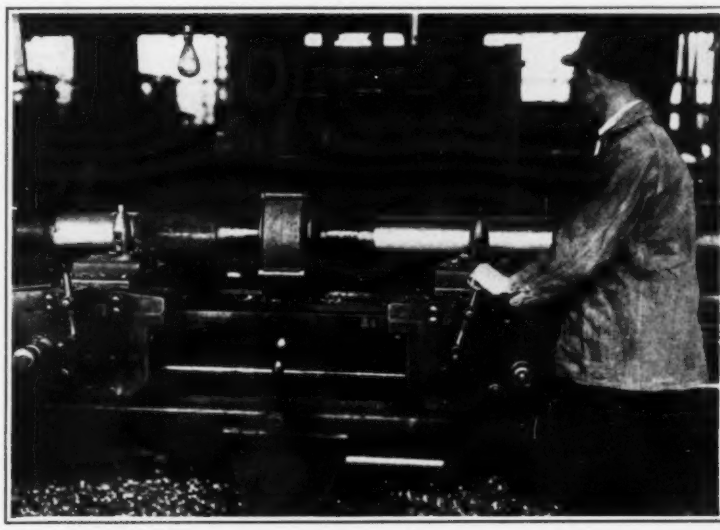
Cutting off forgings for shell bodies to the correct length.



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A twin machine that automatically turns two shell bodies at once.

Polish Music

THE unsophisticated, spontaneous music of the people, military no less than civilian, it is that ultimately leaves its mark upon the musical history of the countries, for the people sing, largely, of the actualities of life as they have experienced it, and their music is gay or sad accordingly.

A good example is cited by Madame Marguerite Walaux in her informative essay, "The National Music of Poland." She says that in this national music is revealed "the soul of this heroic people in all its individuality, agonies, joys, hopes, aspirations, loves and hates, and in its most marked peculiarities." Further, she quotes as outstanding examples Wibicki's "Poland Not Yet Lost," and Ujejski's "With the Smoke of the Fires," the one a kind of psalm, the other a march with something of the atmosphere of the mazurka. A vein of melancholy runs through most of this music, as of the music of Russia, Serbia, Ireland; but it is not the melancholy of despair. Chaikovsky, no doubt, "despaired" in the Finale of his Pathetic Symphony, but, then, you get back to the sophisticated, to the somewhat pumped-up emotionalism of an individual. On the other hand, Madame Walaux, herself a Pole, tells us that not only is there "always something hopeful in its (Polish music) most intense sadness," but also that "the soul of Poland, the inspiration of her melodies, is an impulse toward hope and liberty." No pessimism there! Undoubtedly the Poles are gifted with an essentially musical temperament; I could cite many examples, given to me by Mlynarski and others, had I space.

It must not be thought, as so often it is thought, that Polish music began and ended with Chopin. St. Adalbert composed the hymn, "Boga Rodzica," in the tenth century, from which time there was never a period of any length which had not its characteristic composer. And, if we do not know our St. Adalbert or our Brzozowski (whose Canzionale date from the fifteenth century), we do know something of those characteristically Polish rhythms, the Polonaise, the Mazurka, the Krakowiak, and so on. Of the first of these, incidentally, Madame Walaux gives an interesting, and, to me, previously unknown, account. The germ of the Polonaise, which we all know to be a stately dance, is to be found in the motive of an ancient Christmas song.

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Finished six-inch shells ready for inspection.

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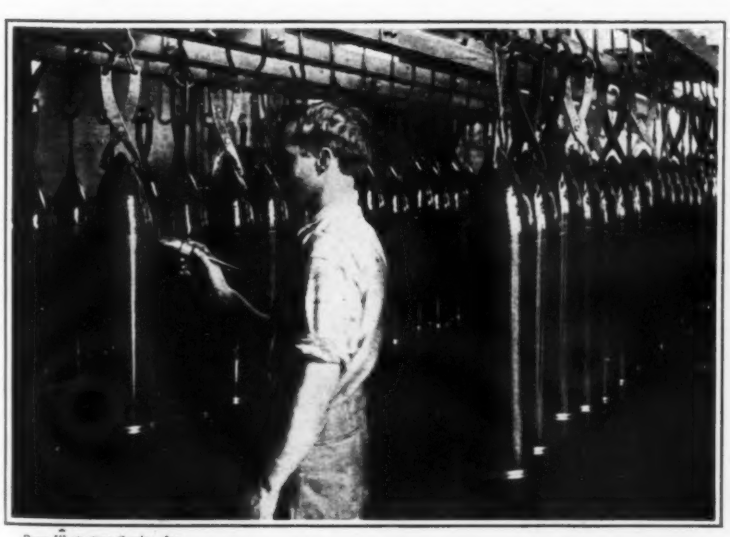
Boring out the interior of a shell body.

The exact date of its invention is unknown, but it would appear to have come from the court. Karasowski declares it to have arrived with the coming of Henry of Anjou, who became King of Poland in 1573. In the following year, he received the representatives of the nation in solemn state at Cracow, the ladies defiling slowly before the king, keeping step to the accompanying music. On the other hand, the Mazur, Mazurek, or Mazurka, rose from the "people," and was adopted by the "classes." Originally it was sung, and was the true national song of Poland, and in Poland is not identical with the dance of the same name. Likewise the Krakowiak, which also is sung as well as danced, hails from the people, and dates from the seventeenth century; while the Dumy or Mumki (reveries) are of much older origin. The Dumka is, or was, familiar in the Ukrain.

Of more or less contemporary Polish music we know all too little, as of Polish musicians. Mlynarski has given us some, Paderewski also, and we know something of Moszkowski, Zelenski, Wieniawski, Rozycki and Stojowski. But even the Poles themselves seem somewhat apt to paint the lily a little excessively—that is, to repeat the familiar glorifications of Chopin, while omitting to instruct their foreign friends as to his predecessors, and, more particularly, his successors.—*Robin H. Legge, in the London Daily Telegraph.*

The Trade Winds and the Temperature of Europe

IN a paper in *K. Akad. Amsterdam, Proc.*, P. H. Galle shows, from observations made at many points, that positive departures from normal in the strength of the northeast trade are accompanied by positive departures of the winter temperature at places to the southeast of a line passing through the British Isles and central Norway, and by negative temperature departures to the northwest of this line. He further demonstrates that, from the data obtained, the winter temperature of northwestern Europe can be predicted with considerable accuracy from a knowledge of the strength of the trades in the previous summer, for a plotting of the elements for sixteen years shows that in fourteen cases the deviations show the same sign, and consequently a very good forecast could be made.



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The finished shells are given two coats of enamel paint.

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Final inspection and weighing. Packing case for two shells shown at right.

The Correction of Echoes and Reverberation in the Auditorium at the University of Illinois

THE work described in this bulletin may be considered as a continuation of an earlier investigation on "Acoustics of Auditoriums."

The interior approximates a sphere cut off on the lower surface by the sloping floor of the room. There is a balcony, but no gallery. The balcony projects twelve feet over the main floor at the sides and thirty-four feet in the rear. The stage is built out into the room instead of being set back behind a proscenium arch as originally designed, the stage house having been omitted to reduce the cost of the building.

The domed ceiling is supported on four equal arches, and the side walls above the gallery are double curved surfaces. There are no windows in the room, the daylight lighting being exclusively through a ceiling light thirty feet in diameter in the center of the dome.

The results set forth in the previous bulletin are briefly as follows: A systematic investigation of the acoustical properties of the Auditorium at the University of Illinois was carried on for several years. "Cut and try" methods of cure were avoided. It was shown by theory and experiment that the usual acoustical faults in a room are due first, to a reverberation, or undue prolongation of sound, and second, to echoes; both of these defects being caused by the reflection of sound from the walls. Various methods of cure were considered—the effect of padding and paneling the walls, the possible advantage of installing wires and sounding boards, and finally, the action of the ventilating system. The conclusion was drawn that the most effective cure lay in padding the walls with materials which absorb sound.

An experimental diagnosis of the acoustical properties of the Auditorium was made. This was done by tracing the path pursued by a small bundle of sound when it was sent in a definite direction and noting what became of it after reflection. Several methods of tracing sound were tried before a suitable one was found. A ticking watch backed by a reflector, or a metronome enclosed in a box having a definite horn gave definite data. However, a hissing arc light with a parabolic reflector was much more satisfactory and gave conclusive results. Enough data were secured in this way to show the general behavior of the sound in the room and also to indicate how the chief echoes were set up. Attempts were then made to secure satisfactory acoustics by hanging curtains and draperies at critical points suggested by the diagnosis. This result was finally secured by suspending four large pieces of canvas in the dome.

From the acoustical standpoint, the Auditorium was then in a much improved condition. The canvas, however, was very unsightly and did not accord with the architectural features of the room. It was therefore proposed that the materials used to correct the acoustics be installed in such manner as to remedy this fault. It was also proposed at this time to install a pipe organ, to decorate the interior of the room, and to change the lighting system.

It was desired that the materials used to correct the acoustics be installed in such manner as to conform with the architectural features of the Auditorium. This introduced a new problem since in the provisional cure the canvas sheets in the dome hung with very little conformity to the curvature of the walls. A further complication appeared when it was found by calculation that the amount of material necessary to correct the reverberation was insufficient to pad all the walls that produced echoes. It was desirable to eliminate the echoes, but it was regarded as risky to install too much sound absorbing material, owing to the danger of making the Auditorium too dead for sound.

Because of these difficulties it was decided to carry on further experiments and to secure more data before deciding on the final cure. Accordingly, one large curved wall was covered with strips of one-inch hair felt, thirty inches wide, placed vertically and thirty inches apart so as to leave bare spaces between them. This arrangement was satisfactory for several reasons; it did not change the curvature of the wall; it used only half the amount of material necessary to cover the entire wall; and because of diffraction and interference effects, it was theoretically more efficient in breaking up the reflected sound than if the same material were spread continuously over the whole surface. Although encouraging, the results were not so marked as expected in diminishing the echoes.

On the basis of this experiment, plans were made for covering other walls in a similar way, except that the hair felt was to be mounted on wooden ribs built out from the wall surface. Such an installation seemed more likely to break up the incident sound than the first plan of mounting the hair felt snugly against the wall. The sound wave on striking these outer felt strips would suffer partial reflection and change of phase, while the remaining portion of the sound would pass through the open spaces and be spread out by diffraction and reflection from the walls. The hair felt strips would oppose the incident and reflected waves, thus breaking up the original sound and diminishing its intensity and possibility of producing echoes.

Because the scaffolding erected for the use of the workmen interfered with the passage of sound waves, the efficiency of this method of placing the felt could not be tested step by step as the material was mounted. The test was deferred, therefore, until the installation was completed. In the meantime the pipe organ was installed, the interior was redecorated, and the lighting system changed, so that only the combined effect of all these factors on the acoustics could be investigated.

The organ was mounted in a unique way by dividing it into two parts and placing them in lofts twenty-four feet above the ends of the stage with a distance of seventy-five feet between centers. This arrangement placed the organ at a considerable distance above the audience. The absence of any vertical surface between the lofts and the audience room prevented any visible arrangement of the organ pipes, but the necessary free exit of the sound was provided for by the construction of ornamental plaster grills covering the pendentives on either side of the stage.

The hair felt was mounted on thin furring strips which were bent to fit the curvature of the surfaces. The dome above the arches and the double curve side walls and single curved rear wall above the balcony were padded in this way. The felt was mounted in vertical strips on the west side wall.

On the east balcony wall the felt was mounted on wooden ribs so that it stood concentric with the plaster surface at a distance of one foot. Eighteen inches below the edge of the skylight in the dome radial strips of felt which approached the wall until they touched at the crown of the arches, were mounted on wooden ribs. The hair felt used was the Akustikos felt developed especially for correction of acoustical faults under the direction of Prof. Sabine.

The modification of the lighting system involved the elimination of the suspended fixtures. The wall brackets were retained, but the main lighting was changed to a semi-indirect system with reflectors above the arches and around the skylight. An ivory tone was selected for the basic color in the redecoration. Ornamentation was stenciled and painted on the various walls and surfaces to give a unified effect. With the exception of the ornamental borders the rep covering the padded surfaces was left its natural color.

The changes in the decoration and in the lighting system would produce little effect.

Tests were made in several ways to determine the presence of echoes. The opinion offered by auditors that the echoes had generally disappeared was, of course, the most satisfactory evidence. One test was made by talking through a megaphone toward different walls. The sound was generated inside a small house and its direction of propagation controlled by two megaphones, one being pointed toward an observer and the other toward a wall which previously gave echoes. No distinct echo could be obtained by speaking simultaneously into the two megaphones. The ticks of a metronome produced very little additional effect, but when a sharp intense metallic sound was tried, echoes were obtained from the unpadded walls but only faint responses from the padded walls. The intense hissing sound of an arc light backed by a parabolic reflector gave more pronounced results. It showed that the padded walls produced a marked effect in reducing the intensity of the sound.

The cone of incident sound received by each pendentive in the rear dome surface is small and, after reflection, spreads over a large area. It was therefore anticipated that little disturbance would result. This prediction was not entirely correct since the echoes reported by auditors, so far as could be ascertained, came from these two walls. An echo was perceptible when the speaker faced directly toward one of these pendentives so that the profile of his face was seen by an auditor seated at one side of the auditorium. The direct sound coming to the auditor was then diminished while the reflected sound was augmented, thus producing an echo.

Other unpadded walls, notably side walls under the balcony, still set up concentrations of sound.

The installation in an auditorium of considerable sound absorbing material eliminates the objectionable condition of satisfactory reverberation being wholly dependent on the sound absorbing power furnished by an audience. This means that rehearsals without an audience can be conducted satisfactorily and that a speaker addressing a small audience is not obliged to contend with a distressing reverberation.

The theoretical advantages in absorbing and breaking up sound waves when hair felt is mounted out from a wall instead of placed snugly against the surface do not appear to be so great as expected. Observers listened to sounds reflected from both types of surface and concluded that a surface having the hair felt mounted out from the wall was more efficient. The conclusions, however, should be checked by quantitative, instrumental measurements since the ear is inaccurate in its estimation of the comparative intensities of different sounds.[2] It appears that the felt is more effective when mounted out from the wall, but there is some question whether or not the advantages secured justify the additional expense of installation and the greater risk of fire.

The music of the pipe organ emerging in large volume from the pendentives in the dome introduced concentrations of sound different from those set up when the source of sound was on the stage. This made it desirable to pad other walls in addition to those requiring padding for the single source of sound.

The effect of the organ music confirmed one conclusion set forth by Jäger;[3] namely, that the strength of the source of sound for good acoustics should be in correct proportion to the volume of the room. It appears that the Auditorium is too small for loud organ music since the sound in this case becomes unpleasantly intense. On the other hand, it appears that the volume is fairly well suited for softer organ music and for a weak source of sound, such as a speaker with a moderate voice. In this connection Jäger contends that an auditorium is limited in its acoustical possibilities; that if a room is too large, it is impossble to make it satisfactory for weak sources of sound. He points out also that the problem of correcting faulty acoustics must include a consideration of intensity of sound as well as of reverberation; that is, the variable factors at command, the value and absorbing power of the room and the source of sound, must be so proportioned as to give not only a suitable reverberation but also an acceptable intensity of sound. He discusses the limitations in obtaining this desired result.

Another deduction made by Jäger which applies rather directly to the Auditorium is that the ratio S/W should be large for good acoustics, in which S is the total surface of walls, furniture, and fixtures struck by the sound and W is the volume of the interior. Theoretically, this ratio is smallest for a sphere, and, since the Auditorium approximates a hemisphere, the excessive reverberation might have been predicted.

Reverberations and echoes were corrected simultaneously by installing a suitable amount of hair felt on the walls which produced echoes. To locate these walls, a new method was developed in which the source of sound was an arc light as explained earlier in this bulletin.

The investigation showed that curved walls are worse acoustically than plane walls since they produce undesirable concentrations of sound and echoes. It also appears that the openings in the pendentives for the organ music and the ventilation openings act similarly to open windows and thus reduce reverberation and diminish echoes.

One acoustical disturbance which was not corrected was that due to talking and walking in the foyer and on the stairs immediately outside the Auditorium. The sounds of footsteps and the reverberation caused by loud talking and accidental noises in the foyer could be reduced by covering the stairs and foyer with a yielding material, such as cork, and by padding some of the walls.

It is apparent from this discussion that the means employed to correct the acoustics, as exemplified by this complex problem, were based upon established scientific principles and this investigation and others of like nature have served to a large extent to dispel the mystery surrounding the action of sound in auditoriums.

Powdered Coal Convenient for Locomotives

ONE feature of the use of powdered coal as a fuel for locomotives is the saving that can be effected when a stop of considerable duration occurs. In such cases the fire can be turned out and restarted again without any outside assistance after standing several hours.

[1] *Bulletin* No. 73, of the Engineering Experiment Station, University of Illinois. SCIENTIFIC AMERICAN SUPPLEMENT, No. 3031, p. 358, and No. 3132, p. 380.

[2] Rayleigh, Scientific Papers, vol. ii., p. 132.

[3] *"Zur Theorie des Nachhals,"* Sitzungsberichtend er Kaiserl. Akademie der Wissenschaften in Wien. Matem-naturw, Klasse; Bd. CXX., Abt. IIa, Mai, 1911.

On the Natural Method of Learning*

By Sanford A. Moss

It is usually assumed that the proper method for instruction of a class in a given theorem is a rational deduction, beginning with previous principles and proceeding logically, step by step, until the final result appears. The statement that this is not the best way will probably be considered very surprising. Nevertheless, the heretical declaration is made that the best plan for giving instruction either in a lecture or a text-book is initial arbitrary statement of the net result, followed by applications and examples, and last of all, logical deduction of the result from previous principles.

This procedure is the best one to follow in all branches of mathematics and physics, and probably in most branches of engineering. It puts the pupil in possession of a subject with the least expenditure of his mental effort. It probably requires greater mental effort on the part of the teacher than the process of rational deduction leading up to the final result. It is the most natural method from the point of view of the learner, even though it may be an unnatural one from the point of view of the teacher.

The easiest and laziest procedure for the teacher is to start out with well-known principles and in the presence of the class proceed logically step by step until the final result is reached rationally. This method requires no preparation and the teacher must simply choose a proper starting point and have a general idea of the direction in which he is to proceed. He can then put equation after equation on the blackboard, each one deduced from the preceding one, and he is certain to arrive at the proper final result. During all of the deduction the teacher knows where he will end, and knows the bearing of each step on the final result. The pupil has no such advantage. He knows nothing of the object sought in the discussion, nor the reasons for the various steps, but must keep his interest sustained until the end without knowledge of where he is going.

As an illustration, I have opened at random a standard text-book on thermodynamics and find "Theory of the Injector." This starts with the expression for the heat energy in one pound of initial steam and proceeds step by step, with equation after equation, each deduced from the preceding one, until finally an expression is arrived at, giving the ratio of the water pumped to the steam used. Then follows an example in which usual numbers are inserted in the formula and a numerical result computed. The reader has no idea as he starts in the discussion as to what its purpose is, or why it is being made. Probably most of us in beginning such a section of a book would hastily glance over the discussion and see what sort of final result was arrived at, and then go through the deduction. Hence, the best way to give this theory would be to start with the statement that the ratio of the water pumped to the steam used by an injector will be shown later to be so and so, and follow with the numerical example and finally give the deduction of the formula.

This method would at once put the pupil in possession of the object of the discussion, and of the principal fact in the whole matter, which is, that it is possible to deduce an expression for the ratio of water to steam. The numerical example would next give the pupil an appreciation of the various quantities involved. Then it would be easy to understand the deduction of the various steps leading to the formula first given.

One very important result to be attained by study of any subject, is general training in the development of a theory, so that the pupil may become able to make similar developments for himself. From this point of view also it is best to have the goal clearly before the pupil, since this gives him at all times during the deduction a clear understanding of the exact meaning of the various quantities involved, so he can realize the reasons for their use.

The older text-books in geometry are good examples of the methods here proposed. They started out with a complete statement of the theorem which was to be demonstrated, and with a figure with all of the steps completely shown. For instance, I have opened a geometry at random and find the familiar proposition, "The sum of the squares described on the two sides of a right triangle is equivalent to the square described on the hypotenuse." Then follows a right triangle with the angles labelled A, B, and C, and with a perpendicular dropped from the hypotenuse to the opposite angle, C. Then follows the statement, "Let A, B, C, be a right triangle with its right angle at C. Then $\overline{AC}^2 + \overline{CB}^2 = \overline{AB}^2$."

Most of the battle is won right at this point. The pupil has a clear idea of what the writer is driving at, and of the exact result to be obtained. In fact, if the pupil retains just this much of the matter he probably knows as much about it as most of the readers of this article unless they be teachers of geometry. In other words, while all of the readers of this article know the theorem in question, it is doubtful if many of them can prove it. The geometry text-book then follows with the formal proof and ends up with the familiar "Q. E. D."

How ridiculous it would have been if the geometry had proceeded as follows:

"We will now develop an important theorem in right triangles. Suppose we take the right triangle A, B, C. Let us draw a perpendicular from a hypothenuse to the opposite angle. Then we have, etc." Finally, the theorem would be arrived at rationally. Such a method of deduction would keep the pupil in ignorance of the reason for the development and would vastly decrease his power of understanding it. Yet this is exactly the method used in developing most matters in mathematics, physics, and engineering.

The method here advocated is really the one by which we have all made our greatest advances in learning in our early years. We were taught arbitrarily, 3 times 8 is 24, and had to learn it by heart. It is only after we had considerable practice in use of the theorem that we could understand a rational derivation of it. Similar remarks apply to long multiplication, long division, etc. We were taught arbitrarily that the circumference of a circle is found by multiplying the diameter by 3.1416 long before we were taught the reason. Here again it is quite probable that few of the readers of this article, except teachers of geometry, can tell the reason.

Exactly the same principles apply to any of our engineering matters. Take, for instance, the theorem that $\int dQ/T$ taken between two points is independent of the path. If we thoroughly know this principle and know how to use it, it is a minor matter as to whether or not we can deduce it. Once again, it is quite probable that most of the readers cannot now make this deduction. Therefore, the proper way to teach this principle is to start out by stating it arbitrarily, and by explaining just what it means, and how it is used, and then end by giving the logical proof. It is not proper to begin with some of the preceding principles of thermodynamics, and without stating the goal, to proceed step by step until the principle is finally arrived at. As in all previous cases it will be vastly easier to follow the detailed proof, if the subject of the discussion has been firmly fixed upon the attention of the pupil by means of initial statement of the principle, and of examples of its use.

A very important result of the recognition of the principle here advanced will be a casting off of the chains with which most colleges bind themselves in connection with "successive courses." For instance, a pupil cannot take calculus until he has had analytical geometry. He cannot take mechanics until he has had calculus. He cannot take machine design until he has had mechanics. He cannot take steam engine theory until he has had machine design, and so on, *ad infinitum.*

The necessity for such succession is purely imaginary. A pupil will get along at least as well, in many cases, if the procedure is exactly reversed. For instance, in machine design, he can be told arbitrarily that it is shown in mechanics that such and such a thing is true, and then proceed to use this theorem without knowledge of the exact method by which the theorem is proven to be true. He can then, at another time, take up this particular theorem as a part of mechanics and go through the derivation. He will understand the whole matter vastly better through having had experience with it in machine design.

Take, for instance, the formulas for stresses in beams. These are usually first taught in mechanics, and the pupil goes through the derivation hardly knowing what a beam is, and certainly knowing nothing of the immense field of use of the formulas. On the contrary, suppose he first takes machine design and is told arbitrarily that the formulas for stresses in beams are so and so, and is given instruction in their use. At a later date he can take mechanics and go through the derivation of the various formulas. He will be able to do this much more readily through the knowledge of the use of the subject.

It is to be borne in mind that many branches of engineering are carried on by men who are familiar with the various formulas and principles, but who have not been trained in their derivation. For instance, many of our most successful mechanical engineers have come up through the drawing room and have been instructed in the various principles without information as to how they were derived. After they have become expert in the use of such principles the better ones have undoubtedly looked up their derivation as a matter of general interest. Such principles are really tools of the trade, and the most important matter is the instruction in the use of these tools—the training in their dextrous manipulation. The exact manner in which the tools were constructed is of secondary importance. A carpenter learns how to use his wood chisel and his plane, and only as a minor matter finds out how these instruments were made in the factory.

Suppose then that we have freed ourselves from the idea that engineering courses must be given in the traditional succession. Many of these courses then can be given in parallel with good results. A pupil can take analytic geometry and calulus at the same time. In the next year he can take mechanics and machine design at the same time, and so on. In each case where a subject is given not preceded by the traditional preparatory subjects, there must, of course, be definite statement of each principle involved, without any deduction, however. At some time or other, either in parallel or at a later date, the so-called preparatory subject must be given with full deduction of all of the principles.

The point which we seek to make, is that the actual use of principles will render much easier the understanding of their deduction, whenever it is taken up.

*Engineering Education. Bulletin of the Society for the Promotion of Engineering Education.

The Use of Perforated Celluloid in the Dressing of Certain Wounds

By S. R. Douglas, M.R.C.S., L.R.C.P., Lond.

The problem of the alleviation of the pain caused by the removal of dressings which have become adherent to the wound is, at present, of more than usual importance owing to the types of wounds from which so many of the soldiers are at present suffering.

The special types referred to are those which either as a result of the primary injury, or of the operative measures necessitated by the onset of gas gangrene, have lost considerable areas of skin. In such cases the dressings are at times so painful as to act very detrimentally on the patients' general condition. Various methods were tried with the view of solving this problem, the principle of all being to place next the wound surface some non-absorbent material which, however, would allow free passage outward of any discharges and inward of any fluid that it was desired should reach the surface of the wound. After the trial of several materials, it was found that there were on the market sheets of perforated celluloid 0.15 millimeter in thickness, the perforations being 1.5 millimeters in diameter, and there being four perforations to each centimeter. This material, which was rather too stiff to be conveniently applied to the irregular surface of the wound, was found to become perfectly soft and pliable, and at the same time somewhat elastic, after it had been soaked in a 5 per cent carbolic acid solution for a few hours. The carbolic acid solution having been washed away with sterile salt solution, the softened celluloid can be applied to the wound surface and falls at once into all the irregularities, and any suitable dressing can be applied over it. On redressing the wound it is found: (1) That the celluloid lifts off the surface of the wound without causing any pain; (2) that all the discharges from the wound have passed through the perforations, leaving the surface of the wound quite clean; and (3) that the celluloid has regained its original stiffness, thus making an accurately fitting splint, which tends to keep the wounded tissue in a complete state of rest. After the celluloid has been taken off the wound it is cleansed in tepid water and again softened and sterilized by placing it in the 5 per cent carbolic acid solution. A convenient plan is to have two pieces of the celluloid for each wound, one of which is applied to the wounded area, the other being kept in a 5 per cent solution of carbolic acid so as to be ready for use.

This material can be obtained in varying thicknesses, the perforations being of the same size and number, and it has been found that the thicker sheets make an excellent material for the making of splints, especially for those cases for whom continuous or intermittent irrigation forms part of the treatment. For this purpose the sheets of perforated celluloid, having been first softened in 5 per cent carbolic acid solution, are molded to the injured limb. After the celluloid has again hardened it is attached to a metal framework such as may be readily made with the aluminium skeleton splinting supplied in the field fracture-box.

It was at first thought that the wound granulations would tend to grow through the perforations, but this has occurred only in those cases in which the granulations tended to be œdematous, and in these the celluloid is not required, as ordinary gauze dressings showed no tendency to become adherent to the wound surface.—*The Lancet.*

The oak tree split the rock.

The clasp of the ivy.

The strangle hold of the woodbine.

Power of Growth in Plants

By S. Leonard Bastin

WHEN the plant is in an actively growing state the power exerted by the developing tissue is often very considerable. Indeed quite often what these growing cells (so delicate in themselves) are able to perform would seem to be quite incredible if the cases could not be clearly shown to be facts. Many people are inclined to discredit the accounts of what growing fungi can do in the way of weight lifting. The stories are but little, if at all, exaggerated, seeing that any close observer can witness astonishing cases for himself. An instance like the following has often occurred: A few years back in a town in the south of England heavy paving stones were laid down on the footways of the streets. After a while it was noted that some of the stones exhibited unevenness. In a few days several of the stones were lifted completely out of their beds and it was then seen that the movement was due to growing fungi. The largest of the stones was examined, with the result that it was found to measure nearly two feet square and to weigh no less than eighty-three pounds! The resistance offered by the mortar that held the stone in place made the feat of the toadstools even more remarkable. In an accompanying picture is shown a case almost as strange. In a single night a large cluster of cap-shaped fungi came up beneath a fence. The uppermost of the growth forced two boards in the fence right upwards, as can be plainly seen by examining the illustration. Although these planks were not very securely held, yet they were fastened by a few rusty nails, sufficient to offer a considerable resistance while the weight of the planks themselves would require considerable power to displace them.

The strangle hold of climbing plants upon their hosts offers some curious instances of the force of growth. The woodbine (*Lonicera caprifolium*) frequently kills trees by the pressure of its twining stems. A struggle between a woodbine and a hawthorn is shown in an illustration. Many of the branches of this tree were dead, owing to the strangle hold of the climbing plant. The ivies (*Hedera*) as well quite often embrace trees in a fatal clasp. It should be remembered that these climbers are in no sense parasitic, and that the damage they do is entirely of a mechanical nature.

The common bracken fern often exhibits a remarkable force of growth. In the spring of the year the young shoots come up with the stem bent over. Now and again the upper portion becomes entangled in the root of a tree, or some other obstacle underground. So great is the power of growth that the earth is often torn about to an astonishing degree in the efforts of the shoot to free itself.

The growing power of roots is usually very great. Kerner places on record the case of a larch tree that was found growing on the top of some rocks. The strongest of the roots had grown downwards in between a cleft separating two great fragments. In course of time the uppermost stone had been raised to the height of nearly ten inches. It was estimated that the rock lifted did not weigh less than 3,000 pounds. In an accompanying photograph the writer is able to show the case of an oak tree. Here the acorn started its life in some soil collected at the top of a rock. As the tree grew it became imperative that the roots should reach the soil below. No doubt taking advantage of small cracks and fissures in the rock the roots started to grow downwards. To-day the rock is split and cracked in all directions by the force of the growing roots.

The fungi that lifted the fence. A piece of white paper has been put behind the gap.

When a bracken shoot is caught the force of the growth is often astonishing.

Old Organ Builders in England

THE professional organ builders of the sixteenth century were very few, and no biography exists save occasional notes in parish books, and so forth. Following are the names of all, or most, of those of whom we have any record. The explanation is that up to about that time the builders of most of the organs—usually then, of course, in cathedral and other important churches—were the ecclesiastics themselves, who, having acquired the necessary knowledge, employed men of the various crafts concerned to carry out, or assist in, the work. Even into the seventeenth century the professional builders used commonly to transfer their establishments from time to time to quarters, provided under their contract, in the place where the instrument was required. Van Os, who built the organ for St. Nicholas, Utrecht, in 1120, and his followers, Engelbrecht, Strasburg, 1260, and Faber, Halberstadt, about 1360, were certainly priests. In the fifteenth century there were several lay builders: W. Wotton, Oxford, the first known in England, built the organ for Magdalen College, 1489. John de John, 1526-31, and W. Betien, 1537-44, were builders to Henry VIII. and to Mary. White, 1531-35, did work to the Magdalen College organ. John Vaucks, another builder of note, 1533, built for Wimborne Minster. Barbye, 1526, who repaired "the little organ" at Magdalen; John Hanson, and John Schowt, shortly afterwards, and Richard Beynton, and T. Brown, about the middle of the century, were all small builders in Oxford. About that period Robartt, of Crewkerne, let out organs to churches. James Dempsey (was buried at), Doncaster, 1567. Broughe, 1590, "changed the organ" at St. Margaret, Westminster. J. Chappington, 1596-7, built for Westminster Abbey, and for Magdalen College, Oxford; and was otherwise extensively employed. T. Hamlyn, 1613, enlarged the organ of St. James (Query-London, Piccadilly); Gibbs, of London, 1618, built for Dulwich College; and Adam Fortess, 1635, for St. George's Chapel, Windsor. In the current issue of the *Musical Times* I just read of old records at Canterbury Cathedral, which mention William Treasorer, maker of musical instruments to Philip and Mary, as having "mended" the organs there in 1573; he lived in London, 1521-1584, and his assistant, Jasper Blankard, did work to the "great organs" at Canterbury in 1578.—*The English Mechanic.*

A New Bromoil Process*

An Improved Method of Producing Artistic Photographic Prints

Walter Dearden

Last year, having mastered the Paget color process after a hard struggle, I thought I would try the Bromoil process, also described by some of its originators and dealers in the outfits as an easy one. I secured Mortimer's book on the subject, also Sinclair's Handbook, and hunted up in three leading photographic magazines all the notes and articles that have appeared during the last seven or eight years. In this mass of information, while there was a general agreement in method, there were also some discordances and some absolute disagreements, but no one seemed to find any difficulty in the inking. The bleaching process recommended for the bromide print varied a great deal, time and temperature being neglected, the former especially, while the latter varied from 65 to 80 deg. Fahr. with a general agreement that a higher temperature was one of the causes of failure.

Perhaps before I go any further I had better explain the principle underlying the Bromoil process.

A print is prepared on bromide paper in the usual way—some details will be given later—and then the image is bleached out by a suitable solution and washed. The gelatine medium carrying the silver salts on the paper is acted on by the silver reduced by the developer and hardened by it in proportion to its amount. In the shadows, where most silver is deposited, this action is at the maximum; and in the highlights, where it is least or absent altogether, it is at the minimum, and intermediate effects are produced by the intermediate tints in proportion to their depth. As a consequence of this hardening the gelatine loses more or less its property of swelling in water. The highlights if not tinted swell normally, the heavy shadows little or none. We have, then, in place of the silver print an image of it in relief in the gelatine. The next thing is to make that image visible, and this is done with an oil pigment applied to it with a suitable brush. The ink is rejected by the highlights and taken by the shadows and half tones in proportion to their depth. With regard to the relief, it is generally claimed it must be low, barely, if at all, visible. However, with this claim I disagree because, though I am now tolerably skillful with the brush, I can not produce anything but a smudge with a low-relief image.

It occurred to me that if I could make a bleacher that would allow the use of higher temperatures, I could produce a higher relief and perhaps find the inking easier. So then followed a long series of experiments in composition of the bleacher, method of using it, and the effect of varying temperatures of the solutions.

While I am not quite satisfied that I have the best possible composition of bleacher, I have one that is satisfactory. It is necessary to leave the print in it a definite time at ordinary room temperature, then to clear it, complete the bleaching, and, after washing, to raise the relief at a relatively high temperature, which, however, varies with different papers. At the maximum temperature, raising a high relief, the inking is very easy, and the resulting print may have nearly the full detail in the negative. Lower temperatures, though still relatively high as compared to those given by Mortimer and others, give more diffusion and increase the skill needed in inking.

In the description of the process that follows I will at first confine myself to the papers that will stand the maximum temperatures and give a print of full detail. Heavy papers, preferably double-weight, are best. The bromide papers I have used for this include all the Wellington styles including the Platino, which many writers say is not adaptable to the Bromoil process, and the Eastman Royal. The other Eastman papers need different treatment, to be detailed later, as so do all the gaslight papers I have tried. Some of the latter can not be used at all, as the gelatine is tanned too hard in manufacturing.

The best print is one that has all the detail needed in both highlights and shadows, but especially in the highlights, as it is almost impossible to paint in detail on the wet print, though it may be done on it when dry by one having the necessary skill. There should be only moderate contrast and the printing need not be very deep. If one has to print deep to get detail in the highlights, then one must ink for contrast as advised later. It is true that with practice the operator can handle some very difficult prints, but it is better to begin with a technically good one.

*American Photography.

I prefer to develop with amidol—probably dianol will do just as well—and give such an exposure as will allow full development, and fix in plain 10 per cent hypo. Then wash. The print may then be dried and bleached later, first soaking it in cold water for a short time, or bleached at once. I prefer to bleach at once when possible.

THE BLEACHING BATH.

Copper sulphate....	25.7 grammes or 396 grains
Potassium bromide . .	17.2 grammes or 265 grains
Potassium bichro. ..	4.3 grammes or 66 grains
Water	1.153 cubic centimeters or 39 ounces

Hydrochloric acid, a few drops to clear.

If distilled water is used no acid may be needed. Most natural waters contain carbonate of lime, which acts as an alkali; no more acid must be used than will just dissolve the small precipitate. This is important, as too much acid makes the bleaching too rapid, which is not desirable.

Portrait. A. E. Aultman.
Bromoil print by Walter Dearden.

The wet print is immersed in enough bleacher to cover it comfortably, and left in it for thirty minutes. Care must be taken to keep the print entirely covered all the time. Do not treat more than one print at a time in same tray unless a large one. Some papers bleach more than others, but generally the image will be merely changed to a light brown and the entire print heavily stained yellow. Wash in running water or in several changes till the water is colorless. The portion of bleacher used can be returned to the stock bottle and used again.

The acid bath consists simply of water to which has been added strong sulphuric acid at the rate of two drops to the ounce. The print is put in this for a few minutes till the color is started from it and is in solution. Then wash, and the yellow color and most of the image will disappear. To complete the bleaching, put the print into 10 per cent hypo for a few minutes till the image entirely disappears or only the faintest indication is left. Wash, and then the print is ready for the next operation, soaked in cold water and then put in the hot water, or it may be dried and kept till required, which is sometimes very convenient.

The relief is raised in hot water at 160 deg. Fahr. for papers named and to get full detail. There are some technical difficulties about this. The temperature must be maintained or not allowed to decrease more than 5 deg. Fahr. in the ten minutes' immersion required, and it is not advisable to keep the vessel used over a lamp or other source of heat, because the Wellington papers are weighted and sink to the bottom and if heated too much they may stain. The Royal floats generally and the heat causes currents, keeping it at the surface and making continual shaking necessary, because if portions are allowed to surface dry, they will take the ink differently from the rest. The method I find best is to use a large vessel—a granite-ware bowl, the larger the better; it should hold at least three or four gallons—and with that the temperature will not fall much. At the expiration of the ten minutes, remove the print, put in a tray of cold water for a few minutes, then lift and let drain and put on the inking pad.

The inking pad can be bought ready for use, but six thick pieces of blotting paper a little larger than the print will do very well. These should be wet, superfluous water drained from them, and superimposed six deep. They must not be too wet, or water will rise over the edge of the print and prevent the ink from adhering or may rise right through a thin paper and have the same effect. That is one reason why the use of a thick paper is advised. If it is necessary to ink a print right up to one edge—it is better always to leave a margin—put a strip of cheesecloth under that edge. On the other hand, in the case of a print requiring prolonged inking, it may be necessary to wet the pad during the process.

The print being on the pad, the next thing is to surface-dry it. This is done with a loosely folded clean handkerchief, with which it is gently dabbed over till no water shows on the surface when looked at obliquely. The relief should now show plainly; the edges of the print rising above the image between them and the image itself showing varying relief representing light and dark portions. The print is now ready for inking.

For this it is necessary to have the proper stag's foot brushes, a tube of black ink, and one of Roberson's medium. It is desirable to have a full assortment of brushes, as a dry brush comes in very handy now and then, but there should be at least one each of Numbers 12 or 14, 8 and 4 to begin with. A small palette knife and a palette—a bit of old scrap plate glass about six inches square is as good as anything else—must be procured. Later, inks in all colors can be obtained.

For a small print squeeze out of the tube a little ink about the size of half a pea and spread it out on the palette with the knife in a circle about one and a half inches in diameter. Then dab the large brush several times on this ink, and then dab it on a clean part of the palette to even it. If the ink has dried out somewhat and spreads very stiffly, it may be necessary to add to the portion on the palette a very minute drop of Roberson's medium, but generally it comes in good condition ready for use. If when the inking has progressed some way it seems hard to get enough ink on the print, the medium may be added.

The brush being charged with ink and the print in place on the pad, start out at one corner of it, say the lower left. The brush must be held lightly at the end of its handle and in a vertical position between the thumb and the first and second fingers and the print tapped very lightly, with a freshly charged brush. The tapping must be rapid, several taps a second, and at the same time the brush must travel up to the upper left corner. On reaching there, move inward half the face of the brush and start a line parallel to the first and running down to the bottom of the print, then again move the brush inward and start to run up to the top of it and so on till it has all been gone over. In doing this the brush may be replenished if necessary, remembering always to make the taps very gentle at first. As the brush gets drier, the taps may be made rather heavier. At the end of the first covering of the print, there will be very little ink on it, the image will be faintly outlined, and the highlights—sky, for instance—will show little or no change. Go over the print again, filling the brush with ink rather more frequently and running its center between the rows of lines made by the first inking. Then turn the print and run the brush at right angles to its first direction, crossing the lines. By this time there should be considerably more ink on it, though it may still look very light and the detail be lacking. Without recharging the brush start working over it again, giving rather harder quick taps, and gradually the detail will appear and the print take a darker tone. It is a good plan to have a duplicate bromide print before you as a guide, and it will show you where more ink is needed. If the foreground, say, is much too light, charge the brush rather more freely

and, instead of tapping, press the brush down rather slowly and withdraw it rather slowly. This will deposit the maximum of ink and destroy detail, but that can be restored by gentle tapping when enough ink has been deposited.

The only way to learn to ink is to practice, and these hints are meant simply as a guide; every one will learn to get results by his own methods finally, and it is a great deal easier to do than the description of it would indicate. However, it may be remembered that a quick tapping with a partially or wholly dry brush gives detail, and a slow stroke means heavier deposit of ink and loss of detail.

With regard to power to modify the print I will illustrate what can be done by an example. Suppose you have a negative taken, possibly, on an ortho. plate, which gives in a straight print poor planes, the foreground perhaps not dark enough, the middle distance and the distance too dark. The first stages of the inking may be done mechanically, then the foreground should be worked to proper depth, the middle distance should be kept lighter in tone, and the distance still lighter, and every one blending into the other. This is not at all difficult to do, and if one plane gets too dark it can easily be reduced with a dry brush. Working with the high relief there is no danger of spoiling a print by carelessly getting even a dark portion of it too dark. If the pigment will not come off, or is too slow in coming, it may be "hopped," that is, the brush held rather high may be allowed to fall on the spot and be caught on the rebound and this continued till the work is done. This should not be done often, however, as it is hard on the brush. As said above, also, detail can be increased or decreased locally by varying use of the brush.

Anyone who will follow all the directions here given cannot fail to get a good print, though perhaps only a straight one the first time of trying, and will soon learn to modify where needed, and most prints do need a good deal of modifying. I do not recommend the process for very small prints except for practice, but it is the only one for large ones, except, of course, for records. With regard to time consumed: rather early in my experience it took me fifty minutes to ink a 10 by 14. There was a very imperfect sky, too light in the middle and too dark at the corners, but by putting on all the color possible in the middle and keeping back the corners, it was made perfectly even. Many other minor changes were also made.

Some changes are needed when using other bromide papers. Under this are included all the Eastman grades except the thin papers, which are not suited to the process. These require the same bleaching method, but it is better to make the whole process continuous. That is, the print should be developed, fixed, washed, at once bleached, cleared, washed, and the relief raised without any intermediate drying between processes. If not convenient to ink at once, the print may be left lying in the last wash water or in the cold water after raising the relief. The temperature used for this latter process must not exceed 125 to 135 deg. Fahr. The P.M.C. handled in this way gives very good results, but if temperature of 160 deg. Fahr. is used, it gives tremendous relief, which will not take the ink. Also the same happens if the process is interrupted by drying the paper. The beginner had, however, better try the first-named papers until he has gained some experience.

All the gaslight papers I have tried also require a temperature of about 125 deg. Fahr. With a higher one some of them give an enormous contrast—they might be useful with a very flat negative. However, the Bromoil process is best adapted for large work, so the bromide papers are the best to use.

As said before, diffused prints may be obtained by lowering the temperature of the water used to raise the relief. The amount of such reduction will depend on effect desired. I would advise that the 160 deg. Fahr. class be tried at, say, 145 deg. Fahr. and that be reduced further if found necessary. The other papers may be tried at about 120 deg. Fahr. and P.M.C. at 115 deg. Fahr. It must be remembered that much more care in inking will be required for these lower reliefs. Especially a freshly charged brush must be applied very lightly and plenty of dry brushes must be on hand. My present impression, however, is that this plan is most useful where the negative is too hard and gives a print of the same character. Then a moderate reduction of the temperature of the water used to raise the relief may remedy the fault. For the rest, if a soft print is needed, it is better to make negative or enlargement with a soft-focus lens and raise the relief at the high temperature so as to preserve the ease of inking. If one has no soft-focus lens, a thin bit of transparent celluloid, placed before or behind an ordinary lens, will give enough diffusion for most purposes.

At the present time, with many chemicals scarce, it may be difficult to get amidol. Some writers claim they have got good results with prints developed with alkaline developers—those like metol-hydro, containing carbonate of soda or potash—but I have not yet tried them, and there is the chance the alkali may affect the gelatine.

If instructions are carefully followed, there should be no trouble in getting good results every time, but in case a print seems to be inking flat, sometimes throwing it back into water a little hotter than that first used and then inking for contrast will give good results. If there is much ink already on the print, it may be soaked off partially—it will not all come off—with gasoline before returning it to the hot water.

When the print is finished, pin it by the corners to a board or a heavy pasteboard—a box lid is very handy—to dry, and do not attempt to mount it for several days as the ink may smear where touched. Do not varnish it till it is at least two weeks old. Mount dry with a thin streak of liquid glue round the edges but not quite reaching them.

The process is a very interesting one, and it grows on one the more one practices it. I hope many amateurs will try it. It will be found a much simpler thing to do than to describe. In this paper I have tried to cover all points and some of the information the beginner will not need at first, though I advise him to read it all.

ADDENDUM.

The preceding reports results as obtained on paper procured more than a year ago. Samples obtained recently show considerable changes, due, I presume, to war needs.

I have so far been able to get only Platino in Wellington paper. At the temperature previously used to raise the relief—160 deg. Fahr.—the emulsion simply washed off, and one sample of Royal did the same, but a later one, while standing the temperature, would not take the ink. I thought at first the gelatine had been hardened too much, but after a good deal of experimenting I found it was only necessary to lower the temperature of water used to about 125 deg. Fahr. to get perfect results. The Wellington Platino also did much better, but required careful inking.

A later discovery was with a portion of a sample of Royal more than a year old. Eight months ago this needed 160 deg. Fahr. and worked well, but tried recently it would not take the ink. Lowering the temperature to 115 deg. Fahr. gave very good results. A sample of No. 8 P.M.C., also more than a year old, refused the ink when relief was raised at 160 deg. Fahr., but also worked well at 115 deg. Fahr.

Out of this came a method of testing any batch of paper as to its worth for the Bromoil process and which, if followed, will remove all the uncertainty due to the paper employed.

Bleach the print as directed, and then cut it into three pieces. Raise the relief of one at 160 deg. Fahr.; one at 125 degrees, and the third at 115 degrees.

No. 1 shows emulsion washed off and, of course, is useless. It is not washed off—relief is rather high—will not take the ink. Relief is right and ink is taken freely.

No. 2. Takes ink but image is rather flat—needs a higher temperature. Takes ink properly. Takes ink fairly but gives too hard an image—needs lower temperature.

No. 3. Image too flat—needs higher temperature. Image right.

I do not think that a lower temperature than 115 deg. Fahr. will ever be needed, but it might be reduced as low as 100 deg. Fahr. if desirable. If no image can be got at any of these temperatures, it may be concluded the paper on trial is no good for the purpose.

Having found out the right temperature to use to give a good image with an average print, the necessary change to be made for a hard one or a too soft one is easily found; 5 deg. Fahr. will make an appreciable difference.

I am sending a print from a portrait negative made by O. E. Aultman, on Royal paper as now supplied. Data: Print overprinted, development shortened, result desirably soft, relief raised at 125 deg. Fahr. I recommend this paper; it takes ink easily with the right relief; after going over the print twice, it may be applied freely.

The Composition of the Upper Atmosphere

In discussions concerning auroræ, the propagation of sound through the atmosphere and zones of silence, and meteorological and astronomical phenomena, it is frequently assumed that the air does not merely become more and more rarefied, as greater heights above the earth's surface are reached, but that its composition also changes. The heavier elements are supposed to be confined to the lower strata, while hydrogen and helium, hardly recognizable by ordinary analysis in the air we breathe, are assumed to predominate in the higher strata. There is very little observational evidence to support that assumption, though it sounds plausible. The difficulty is to collect air samples of sufficient bulk at great heights. Unmanned balloons (*ballons sondes*) have been sent up to twelve kilometers and greater heights; but these balloons can barely carry the automatic instruments which record the temperature and pressure, and could not be charged with bottles and with automatic devices to open and to close those bottles again. Free balloons have lost in favor since man has learned to fly, and flying machines have rapidly developed into war machines. Some day they will possibly be utilized for scientific purposes. In Europe the exploration of the upper air is interrupted for the present, and even the working out of material collected before the war is interfered with. In 1911 and 1912 Professor A. Wigand, of Halle, made four balloon ascents from Bitterfeld up to heights of nine kilometers (6 miles) and collected eleven samples of air in bottles apparently of suitable construction. They were cylindrical glass vessels of two liters capacity, each ending below in two narrow glass tubes provided with stout walls; one of these tubes was sealed after examination, the other was fitted with a three-way cock to which a long tube of aluminium could be attached. This tube had a length of 30 meters and an external diameter of 7 millimeters; the tube was built up of twenty sections, which were screwed together when wanted. By the aid of a small rubber balloon the air was sucked through the tube, the tap was then turned, and the air admitted into the glass bottle, which was afterward closed. The use of the tube and other measures prevented contamination of the air collected with the carbon dioxide exhaled by the aeronauts and any hydrogen escaping from the balloon. The analyses were to be performed by Professor E. Erdmann by his method of fractionated condensation. Only four analyses could be made before the war broke out, however, and as the completion of the work looks doubtful, the preliminary results were published in the *Physikalische Zeitschrift* of September 1st, 1916. Fortunately, some further data were available. When Halley's comet approached the earth in 1910, balloons were sent up from the observatories at Lindenberg and at Vienna, and twenty-one samples of air were collected; some of these were also analyzed by Erdmann. The analyses consisted of determinations of the carbon dioxide and of the gases helium, neon, hydrogen, and, further, of spectroscopic tests for hydrogen. The results do show that the upper layers of the atmosphere contain less carbon dioxide and more of the other gases mentioned than the lower strata. The carbon dioxide content decreased from 294 to 277 cubic millimeters per liter, and that of the lighter gases increased from 22.8 to 33.7 cubic millimeters per liter, when the height of 9 kilometers was reached. The assumption is thus confirmed that the diffusion in the higher strata is not the same as in the lower strata, and that the heavier gases tend to concentrate near the earth's surface, while the lighter gases accumulate higher up. But there is not sufficient material to draw any further conclusions, and the observed intensification of the hydrogen line in particular would not justify the assumption of a hydrogen atmosphere. Two liters of gas make up less than half a gallon, and that is a very small volume for the analytical determination of traces of gases.—*Engineering.*

Maintaining an Entrance to Liverpool

The big dredging operations at the port of Liverpool began twenty-two years ago, and up to the end of the year under review the huge amount of 243,451,500 tons of sand, etc., has been taken up from the channels and at the bar, the "Leviathan" accounting for 69,703,400 tons against the "G. B. Crow's" 76,951,380 and the "Brancker's" 77,264,450 tons. The "G. B. Crow" and "Brancker," however, were in commission years before the "Leviathan" came on the scene.

During the year the dredging in the river above New Brighton resulted in the removal of nearly 10,000,000 tons, the "Coronation" being responsible for 1,764,770 tons and the "G. B. Crow" for over half a million tons less. In this section of the river the total amount dredged since October, 1893, is 78,335,456 tons. Several of the dozen local dredgers have been employed also in dredging operations in the Manchester Ship Canal. From the Eastham Channels 193,630 tons of silt were taken out to sea, and in the canal itself, between the locks at Eastham and Latchford, 751,970 tons were removed, the total quantity of silt dredged during the year being nearly two and a half million tons.

During the past year 17,209,280 tons of material were removed from the Mersey bar and sea channels.

Road Drainage*

The Importance of Proper Foundations

W. F. Childs, Jr.

In the construction of roads, as in the construction of buildings, the foundation should be the first and chief consideration. Road foundations require drainage, for where rainfall is an item to be considered in road building, drainage becomes one of the fundamentals of road construction. Again, foundation work as applied to road construction takes us back to the consideration of soils and sub-soils. So the writer believes that the sub-soil, properly drained, is the real foundation of the road, for the principal agent in the destruction of the sustaining power of a soil is "moisture" and in order to overcome this "destructive agent" drainage in a scientific way must be resorted to.

In considering the problem of road construction in Canada, the Commissioner of Roads of Ontario laid down three fundamental principles of road construction: First, drainage; second, drainage; and third, drainage. These three principles may apply to road construction in a larger portion of this country where we have sufficient water fall to make drainage a prime consideration.

Road drainage, broadly considered, is a matter of: First, climate; second, topography; and third, soil; and may be treated separately under: First, surface drainage; and second, sub-surface or underground drainage.

In the case of surface drainage, where it is assumed that the surface of modern roads is impervious to water or nearly so, the water can be shed in four ways: First, by cross slope or crown in construction; second, by grade after crown is determined; third, by discharge into natural water-courses; and fourth, by discharge into artificial outlets. Ordinarily the surface water can be taken care of by the first three methods, but in low, flat country it will be found that the fourth method (discharge into artificial outlets) is very important and sometimes the sole means of shedding surface water.

On the subject of crowning modern roads there is a vast diversity of opinion among engineers prominent in highway construction. Some advocate what appears to be an excessive crown, while others have a tendency to reduce the cross slope to a minimum. No hard set rule can be laid down for the crowning of modern roads that would apply in all cases and under all conditions. The crown of the road should be determined by: First, character or type of road; second, by locality; and third, by grade. Natural earth, sand-clay, and shell roads should be given a crown in construction some two inches greater than that which is ultimately desired. This opinion is based upon the fact that these types of road are more susceptible to compaction and flattening under traffic than the more permanent roads of harder surface. For water-bound macadams the generally accepted practice is to have a cross slope of one-half inch to the foot, while for macadams protected with bituminous or tar tops a somewhat less crown can be used. For concrete roads, fourteen and sixteen feet in width, the custom is to have a crown of two inches. Concrete, being a monolithic mass and more dense than other types of road, is naturally less susceptible to the flattening action of heavy traffic, consequently the crown can be so reduced as to be just sufficient to shed the water to the sides.

In thickly populated districts a high crown is dangerous to the traveling public and the cross slope of roads constructed through towns or other thickly populated districts should be reduced to that which is just sufficient to drain the water to the gutter line. In such districts high crowns cause a sliding motion of vehicles and bring an extra strain upon the lower portion of the wheels. This is objectionable and causes public criticism, which reverts to prejudice against modern road construction.

Finally, in considering the proper crowning of roads the importance of the question of grades must not be underestimated. The general practice, within certain limits, is to increase the cross slope as the grade becomes steeper. For all grades up to and including five per cent the crowns mentioned are considered sufficient. In excess of five (5%) per cent grades the cross slope should be increased so as to shed the water to the sides rather than to allow it to run down the surface of the road, or make a curve in its course of final discharge. If we accept the above statement as true, or approximately so, then it is seen that grades play an important part in the surface drainage of modern roads. A flat or level road should not be built. It is better to have a longitudinal fall, no matter how slight. In some localities, and the writer's experience has been confined mostly to the construction of roads in low, flat country, where the cost of borrow material for raising fills is prohibitive, it is almost impossible to build roads with a longitudinal fall without excessive first cost, which naturally causes public criticism. In such cases the writer has resorted to the zero grade and gotten longitudinal drainage by opening deep side ditches. This method has proven very satisfactory, for oftentimes there is a decided advantage in longitudinal drainage rather than continually shedding the water sidewise.

By discharge of surface water into natural watercourses is meant the designing of crown and grade so as to draw the water to a natural stream for final disposition. Provision is made for carrying water under the roadway by means of specially designed pipes or culverts and bridges. The sizes of these openings depends upon width of stream, tide fluctuation, and volume of storm and surface water. The volume of storm and surface water reaching the culvert or bridge depends upon drainage area, character of soil and topography of the surrounding country.

In low, flat country oftentimes an artificial outlet to natural watercourses is the only means of carrying off surface water. By artificial outlets are meant tax-ditches, runways, outlet ditches, etc.

The problem of caring for underground water is first, a matter of soils; second, a matter of topography; and third, a matter of climate. There are many soils of a gravelly, sandy, or similar character, which are, in effect, self draining and do not require particular attention. The difficulty is with those highways built upon clayey soils or those of a retentive nature.

The importance of drainage is recognized by almost all highway engineers and failures are due mostly to the fact that a proper study of the character of the sub-soil has not been made. Again engineers have been slow to make proper provision for drainage, not because they do not realize the importance of same, but owing to the increased cost of construction by the introduction of this all important feature. The engineer, in the preparation of plans and cost data for road construction, has oftentimes been embarrassed by the statement of public officials that a modern road can be built for a stipulated sum per mile, and he hesitates to increase the cost of construction, by the introduction of an extensive drainage system, to such a point as to cause him to be publicly criticised for extravagance.

Where the ground water comes within three feet of the surface of a modern road the water level should be lowered and some system of drainage designed to carry off the surplus water. No doubt mistakes have been made in the drainage of modern roads resulting in, perhaps, many miles of underdrains being built which have been practically useless, owing to the fact that the capillary action of the soil, intensified by the tamping action of the traffic passing over the road, nullifies to a large extent the supposed effect of the drain in lowering the water table. This often occurs, notwithstanding the fact that there may be a supposedly waterproof surfacing on the road.

There are times when an underdrain, properly constructed, is absolutely necessary to carry off an excess of ground water, but just when such a system of drainage is necessary and the cost of construction is warranted is one of the most important problems the designing engineer has to deal with. A most careful study of local conditions, particularly soil and topography, should be made before an extravagant drainage system is installed or recommended. The best policy is to put in extensive drainage where it is absolutely necessary and omit it where there is an element of doubt.

In this connection it might be stated that, oftentimes, in making cuts through gravel or clay hills for betterment of grade, springs are encountered. The first thought is to lower the water level by an extravagant drainage system, while if a little time and consideration were given to investigation it would be found that the first rush of water is merely a lowering of the table to its new level owing to a change in original grades and that the springs disappear entirely within a year.

Sub-soil drainage may be effected in a number of ways, chief among which may be mentioned use of blind-drains, V-drains, tile-drains and side ditches. A combination of tile and V-drains used in connection with side ditches have proven satisfactory in low country. Some engineers prefer and advocate heavier foundations, similar to Telford construction, rather than go to the expense of constructing expensive and, sometimes, uncertain underdrains.

In the construction of underdrains climate plays an important part. The drain should not be constructed until a close investigation of the frost line has been made. The construction of these drains depends so much upon natural conditions, both above and below surface, that no set rules can be prescribed that will hold true for all cases in all localities.

In very soft clayey soils sub-surface drainage may be effected by laying tile under the roadway, at right angles to center-line, at established intervals draining to a system of longitudinal underdrains running to a natural outlet. For the cross-drainage an ordinary soil pipe, four inches in diameter, may be used. This tile should be so laid as to drain both ways from center to the underdrains constructed on sides of road. As the infiltration of the water under the road is by action a horizontal force, while the water passing to water tables acts as a vertical force, a great advantage is had by backfilling the longitudinal drains, parallel to center-line, with crushed stone. The water tending to seep under the road will come into contact with this impervious wall and follow it down to the tile and flow off. If the underdrain is laid in a clayey soil, practically the same result can be had in a cheaper way by laying tile in ditch, previously opened, and backfilling same with clay and puddling it.

The writer's experience with the construction of modern roads in low, flat country, mostly on relocations through lowlands, swamps, woods and cultivated fields, is that for economy and effective drainage, the open side ditch has proven entirely satisfactory. He advocates a ditch starting at a point some five or six feet from edge of surfacing with a slope of 4:1 (four feet horizontal measurement for each one foot vertically) on road side, a flow line from one to two feet in width at bottom of ditch, and a back slope equivalent to angle of repose for the particular soil encountered. This type of ditch insures safety to traveling public, lowers the water level, drains the road and is economical in serving the two-fold purpose of drainage and furnishing material with which to raise the grade of road above that of surrounding country. The use of side ditch is arbitrary and depends upon the relative true elevation of the locality as compared with mean tide.

Some engineers object to the use of the side ditch for drainage purposes on the ground that if the ditch is dug alongside of road the natural plane is broken and a greater surface is exposed through which moisture passes. In the past two years the writer has been in active charge of the construction of some eighty miles of modern highways, including waterbound and bituminous macadams, concrete and bituminous concrete, and shell roads, and the general rule for drainage has been the "side ditch." Once these ditches are properly opened and the slopes kept shaped up and free from vegetable matter and the flow line kept clear, they will perform all the functions of drainage usually had by a much more expensive system of underdrain. The advantage of the side ditch over the underdrain is that the former is always open to inspection and easy of access for repairs, which is not the case with the underdrain.

In the summer of 1914 the writer drained a short piece of concrete roadway by intercepting the water with six-inch porous tile underdrain. By opening a system of holes the direction of seepage was ascertained. A ditch one foot wide on bottom and eighteen inches below sub-grade was then opened at a distance of nineteen feet from center line of road and running parallel with same to a natural outlet. On the bottom of the ditch was laid a shallow depth of straw and the porous tile laid directly upon the straw. On the top of the tile were laid longitudinal strips of two-ply tar paper twelve inches wide, joints overlapping four inches, and the pipe covered with the clayey material excavated from the ditch. There were some 400 linear feet of this tile laid at a cost of less than eight cents per foot in place, including excavation, straw, tile, tar paper

*The Cornell Civil Engineer.

and labor back-filling and laying tile. Previous to the laying of this tile the shoulder of road was always soft and mucky and water always stood along edge of concrete, which had begun to show disintegration. Since this tile was laid no further trouble has been experienced. The writer believes that very good results can be obtained by use of porous tile in sub-surface drainage and that very often the glazed tile with bell joints is used at much greater first cost when the ordinary farm tile would have answered the same purpose. Conditions being the same the life of the glazed tile is somewhat longer than that of the porous tile, but the cheaper first cost of the latter will more than offset the difference in serviceability of the two.

From observations made during the clearing rights of way for new construction along relocations where dynamite was used for removing tree stumps, the speaker has noticed that the explosion of dynamite tends to improve drainage conditions, particularly in heavy soils of a retentive nature. The reactionary force of the explosion opens up the underlying strata of hard pan and allows the water to pass through to a lower level and finally into the side ditches which have been opened for sub-surface drainage. The writer recalls two instances of clearing relocation through pine timber where the character of soil and natural conditions were practically the same. In one case the contractor used dynamite for removing the tree stumps while in the other case the trees were removed by the use of a stump puller. In both cases drainage was effected by side ditches and it was very interesting to notice how much sooner the sub-grade was cured in the first case than in the latter, showing that the use of dynamite hastened the action of the ditches in draining the road.

In road construction it is very important that the grading and drainage should proceed ahead of the surfacing. This is important in all cases, but most particularly where the road follows a relocation over virgin soil and through virgin timber where there has been no attempt at drainage. In the latter case it requires time for the newly constructed drainage system to fulfill the purpose for which it was designed and should be carried out ahead of the rest of the work. This is not only a great benefit to the road itself but a decided advantage to the contractor in case bad weather is met with during construction.

The writer wishes to give the following extracts taken from reports on road constructions by prominent English and American highway engineers:

Mr. R. O. Wynne-Roberts, a prominent English engineer, has the following to say concerning road drainage:

"The first consideration in connection with all roads is that of sufficient drainage, but unfortunately there are hundreds of miles of highways without satisfactory means of draining off the sub-soil water and afterwards of conveying it away; the same remark often applies even to surface water. In the case of water-bound roads—and those constitute the principal portion in this country—the presence of water or excessive moisture keeps the sub-soil in a sodden condition, thereby so reducing its sustaining powers as to make it unable to bear the concentrated loads often imposed, with the result that the metal surfacing is deformed, disintegrated, and in some measure pressed into the soft sub-soil. At the same time the displaced sub-soil oozes upwards, causing the roads to be softer than before, muddy in winter, dusty in summer, expensive to maintain, and giving rise to dissatisfaction to the road authorities who maintain, and to the public who use the highway in question. Economy of maintenance of a public road is largely governed by the condition and character of the sub-soil. It is highly desirable that its weight carrying capacity should be preserved and improved by efficient drainage."

Mr. Jack, County Surveyor of Heresfordshire, England, has this to say in his Fourth Annual Report in 1911:

"The trunk roads have not in many cases a crust thicker than four inches and this crust rests directly on the clay sub-soil. It is possible with efficient underdrainage and impervious surface this clay may be kept sufficiently dry all the year round, and if so, then the many troubles arising from the yielding clay may be surmounted. I do not, however, consider this a certainty with a water-bound crust, however well the underdrainage is carried out. The very fact of the permeability of the surface would cause the underlying clay to yield under heavy weights in wet weather. The principal function of the drains would be to get rid of the underground water and not so much the water which falls on the surface. This can be quickly disposed of through the grips and ditches if the surface is waterproof. I do not by this suggest that underdrainage would not vastly improve the condition of the water-logged lengths of road made under our present system. On the contrary, I have cases in mind where I know the result of deep drainage would be most beneficial, and would certainly prevent the roads giving way as they did in many cases last December."

Mr. Frank D. Lyon, Second Deputy Commissioner of the New York Highway Department in 1910, stated, in a paper on the "Location and Drainage of Highways":

"Among the road builders of to-day good drainage is recognized as one of the most important considerations, whether the roads in question be of earth or those with a covering metal. No one subject involved in the construction of an earth, gravel or macadam highway is of as much importance as that of drainage."

Mr. A. W. McLean, Provincial Engineer of Highways, of Ontario, Canada, states:

"Roads in Canada to-day are bad for the same reason they were bad in England a century ago, before the time of Macadam. They are drainless quagmires, swallowing the stone and gravel placed on them. Townships generally spread stone on their roads and speak of them as being 'macadamized.' To macadamize our roads means, in the first instance, that we must thoroughly drain them by surface and underdrainage. The essential principle of a macadamized road is drainage. This was the principle advanced and introduced by Macadam, and it is the one commonly neglected throughout Canada to-day."

Some of the more common indications of a poorly drained road are:

1.—Frost thrown surfaces.
2.—Wet spots on the surface.
3.—Mud up through the surface.
4.—Surface disintegration.
5.—Visible wheel tracks and horse paths on surface.

Finally there is no truer statement than the old adage: "That a road, like a house, should have a dry cellar, a firm foundation, and a tight roof."

Manufacture of Bar-Le-Duc Jelly

BAR-LE-DUC jellies and jams take their name from the town of Bar-le-Duc, capital of the Department of Meuse, which specializes in their manufacture. They are prepared with currants specially chosen on account of their size, but which are not produced by any particular variety of currant bush. The following is an outline of the method of preparation generally followed:

During the month of July each year trained workers receive from the factories quantities of currants, which they take home for the purpose of removing the seeds. In this process the berry is held in the fingers of one hand and the seeds are removed by means of a goose-quill sharpened to a fine point. The work is exceedingly difficult, and requires considerable dexterity, acquired by long practice. As soon as the currants are returned to the factory, sugar is added and the fruit cooked. The quantity of sugar used is much greater than in ordinary jams and jellies, owing to the fact that the jelly boils for only a short time, in order to avoid the oversoftening of the berries. (Softening would cause them to lose their attractive appearance, which is the specialty of the Bar-le-Duc product, the whole berry being seen through the glass jar.) When prepared, the jelly is placed in small pots closed with a metal cap, and the pots are placed in boiling water to ensure the keeping qualities of the product. As Bar-le-Duc jelly is prepared chiefly for the export trade, this latter is essential; but as the district where it is prepared is close to the war zone, it is probable that little of the jelly is being made at the present time.

The Repair of Bronze Castings or Forgings by Welding

As a result of experiments made at the United States Bureau of Standards on the effects of making repairs on bronze castings by welding, or burning in, described in Technological Paper No. 84, by Paul D. Merica, assistant physicist, and C. P. Karr, associate physicist, the following conclusions were deduced:

1. That the welding in of constrained portions of castings (forgings, wrought articles, etc., naturally, as well) of manganese bronze, produces, in general, local initial tensional stresses within and near the burned-in zone, of value equal to the true elastic limit of the material unless the shape of the casting is such that extensive distortion may occur.

2. That such castings should therefore be either preheated carefully for welding, such that all parts of the casting cool down together from a dull red heat, or the casting should be subsequently annealed. Experience indicates that a low temperature anneal is sufficient for this purpose—e. g., from 400 to 500 deg. Cent. (760 to 940 deg. Fahr.)—for from one to two hours. Either of these precautions should eliminate these local stresses resulting otherwise from the burning in and should produce castings free from danger of subsequent cracking.

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